

National Institute on Alcohol Abuse and Alcoholism

RESEARCH Monograph-3

**Normative Approaches
to the Prevention of
Alcohol Abuse and
Alcoholism**

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National Institute on Alcohol Abuse and Alcoholism

RESEARCH MONOGRAPH SERIES

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3. Normative Approaches to the Prevention of Alcohol Abuse and Alcoholism

Editors: Thomas C. Harford, Ph.D., Douglas A. Parker, Ph.D., and Lillian Light.

Proceedings of a symposium, San Diego, California, April 26-28, 1977. Papers describe the theoretical and empirical frameworks of the socio-cultural and distribution-of-consumption models of drinking behavior and assess their implications for strategies of primary prevention.

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NORMATIVE APPROACHES TO THE PREVENTION OF ALCOHOL ABUSE AND ALCOHOLISM

Proceedings of a Symposium
April 26-28, 1977
San Diego, California

Sponsored by:

Laboratory of Epidemiology and Population Studies
Division of Intramural Research
NIAAA

Edited by:

Thomas C. Harford, Ph.D., Douglas A. Parker, Ph.D., and
Lillian Light

U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Public Health Service
Alcohol, Drug Abuse, and Mental Health Administration

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Foreword

Two theoretical explanations of alcohol consumption patterns and their respective implications for prevention were the key issues of the presentations and discussions at the symposium upon which this publication is based. Those who participated in the symposium did much probing into the two major models to identify their strengths and weaknesses. The various views expressed at the symposium reflected the current knowledge and insight of those attending and may provide the basis for new, additional, or alternative approaches for prevention.

This volume, No. 3 of the *Research Monograph Series* of the National Institute on Alcohol Abuse and Alcoholism, should thus provide an excellent foundation for improved research in the future by stimulating further discussion and investigation into the causes of alcohol abuse and the opportunities for prevention.

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Preface

A 3-day symposium on Normative Approaches for Prevention of Alcoholism and Alcohol Problems was held in San Diego, Calif., April 26-28, 1977. The purpose of the symposium was to examine the theoretical and empirical frameworks of current approaches to alcoholism, to identify their implications for policy, and to stimulate the integration of current research knowledge for the prevention of alcohol problems. The first day of the symposium was devoted to an examination of the sociocultural model; the distribution of consumption model was discussed on the second day of the symposium; and on the third day attention was focused on the research and policy implications of both models.

This monograph incorporates the papers presented at the meeting and portions of the general discussions held after the presentation of papers. The verbatim transcript of the discussions was jointly edited by participants and editors to capture both spontaneity of response and comprehensiveness of the ideas generated during the 3 days.

The exchange of ideas that took place after the presentations showed deep concern with the difficulties involved in the prevention of alcohol problems. Excellence in prevention programs is possible with a strong foundation in research and informed guidance by men and women with ideas and reason.

Dr. M. H. Brenner provided the comprehensive bibliography which appears as an appendix. Brenda Yankey rendered technical assistance.

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A Critical Review of the Sociocultural Model of Alcohol Use

Dwight B. Heath

With reference to the study of uses and abuses of alcohol, the sociocultural model¹ emphasizes patterns of belief and behavior that characterize various populations. Because members of different societies hold different beliefs about alcohol and its uses, effects, and related actions, as well as about the meanings of all these factors, people in different societies drink in different ways. Moreover, this behavioral variation appears to be related to differences in the nature and frequency of problems associated with drinking in those populations.

With respect to the prevention of alcoholism, the sociocultural model emphasizes efforts to modify patterns of belief and behavior about alcohol and its use in ways that might lessen the frequency with which associated problems occur.

Theoretical and Empirical Bases of the Model

The sociocultural model derives from the view, now widely accepted in the social sciences, that human behavior is the complex resultant of an interplay of biological and historical factors, including interaction among systems that can be distinguished as those of the culture, the society, and the individual.² The theoretical basis is rarely spelled out in great detail, but tends to be eclectic, combining learning theory and modified psychoanalytic components in relation to the individual with structural and functional emphases in relation to society and culture.

¹“Sociocultural model” is a convenient label that has gained some currency in the literature on alcohol during recent years. The term is used in a peculiarly restricted sense with reference to alcohol studies.

²Because this audience is already sufficiently familiar with the relevant literature, emphasis will be on selected trends rather than on specific illustrative examples.

In large measure, authors who use the sociocultural model with reference to specific populations start from empirical descriptions of behavior and offer ad hoc analyses in an inductive manner. Those who use it for purposes of large-scale cross-cultural comparison sometimes formulate their hypotheses in the manner of deductive reasoning, but even they must rely on data that were collected by others without those hypotheses in mind.

In order to appreciate how and why some of the strengths and weaknesses of the sociocultural model occur, it is important to consider the methods on which such research is based. In this respect, there is ample room for controversy, which I shall here address only in summary fashion. Most of the studies are written by anthropologists on the basis of field work in alien societies or by sociologists on the basis of surveys among minority populations. Because the anthropological style is less well known, it deserves brief special comment. Traditional anthropological techniques include a combination of participant-observation and interviewing, with greater emphasis often on nondirected, rather than directed, interviewing and with little attention to any kind of systematic sampling procedures. Although few claims are made for scientific rigor, there is often remarkable consensus among several skilled observers in a given situation, and the inability to replicate often seems relatively inconsequential considering the fullness and richness of behavioral and attitudinal data that can be observed, elicited, and recorded in a natural (that is, sociocultural) context.

There are many who feel that questionnaire surveys provide much more precise, quantifiable, and comparable data with respect to attitudes and behaviors, but a variety of logistical and cultural factors makes it difficult if not impossible to apply standard survey techniques in many populations. Even in situations in which surveys might be used, the advantages of reliability must be weighed against those of validity. It is commonplace that views held by others about alcohol often differ markedly from what might have been foreseen by an investigator, just as actual behavior patterns often differ markedly from what people will report, especially with respect to a topic such as drinking (which is often hedged about with strong emotional and evaluative concerns, not to mention legal restrictions).

Most of what I have said about anthropological field methods is just as pertinent to segments of urban, literate societies as it is to smaller, relatively isolated, and more vividly different tribal and peasant societies that still remain outside the mainstream of influence from modern industrial nations. This is not meant to imply that techniques that emphasize qualitative rather than quanti-

tative data collection are in any general sense better, but to indicate that they are not necessarily worse. In this context, it is not irrelevant to note that the appropriateness of research methods depends largely on the order of the questions being addressed. Furthermore, although it may appear a truism to this audience, I feel impelled to stress that the very dialectic I have introduced here for the sake of convenience is problematic in itself—there is no reason why a variety of research strategies should not be applied together, or by turns, in a given situation, yielding perspectives that should be complementary in providing more and richer insights into the complexities of human behavior.

Still, it would be misleading if I were to give you the idea that the diverse, scattered, and rapidly increasing body of information on sociocultural aspects of drinking has been carefully compiled, with attention to what techniques are most likely to elicit what kinds of information in what settings. On the contrary! A feature that sets alcohol studies apart from many other behavioral and historical studies is that most of the pertinent data and observations have been serendipitous by-products of research on very different foci of concern. Until a few years ago, no social scientist had gone into the field with the intention of studying alcohol in its natural context.³ Because of their skill, patience, and thoroughness, such observers collected and were able to report in rich detail on aspects of alien cultures that were tangential to their major investigatory concerns. (Unfortunately, such studies are in no sense replicable, and in very few instances is there even much hope for meaningful comparability between them.)

Although anthropologists have contributed most to the descriptive corpus on drinking patterns in sociocultural context, it is noteworthy that sociologists and psychologists, using very different methods, have more often contributed in ways that lend conceptual weight and theoretic plausibility to the sociocultural model. A benchmark in any discussion of this subject is Bales' study (1946), largely based on historical, religious, and demographic documents. Horton's contribution (1943) may have been at least as important in pioneering large-scale cross-cultural correlational analysis of behaviors as in offering a generalized answer to the question of why people drink. The work of Jessor and his associates (1968) combined measures of personality with social

³For a review of studies on interrelations of alcohol and human behavior in various cultures, see Heath (1976); for a more detailed review of the conceptual and theoretic concerns of sociocultural studies of drinking, see Heath (1975).

and cultural variables in different populations, and a few other efforts have been made to systematize the complex data.

What Contributions Have Been Made?

In a sense, social scientists have to rely heavily on natural experiments because there is little opportunity to manipulate variables at will. This is not as severe a limitation as some observers might believe, however. Among scholars who emphasize sociocultural perspectives on alcohol and human behavior, the greatest contribution has probably been the substantive one of expanding our knowledge about the range of variation that occurs throughout time and space. Ethnographic accounts from around the world have revealed some extreme patterns of belief and behaviors associated with drinking that have extended our understanding of the range of human possibilities. Similarly, historical accounts show how attitudes and values, as well as ways of drinking, have changed in various ways and at different rates in many cultures.

It was less than 40 years ago that alcohol studies first recognized the now-obvious fact that the effects of ethanol include social and cultural factors as well as chemical, physical, and biological factors. The malleability of both the substance and the human organism is greater than had been imagined, and the substance-organism interaction takes on wholly new dimensions when one pays attention to the fact that drinking is in large part a social art. "Drinking" (used here as a convenient code-word for the ingestion of ethanol) almost invariably involves other complex forms of behavior. Even in these few societies where solitary drinking is permissible, it is inextricably imbedded in a matrix of social and cultural factors such as values, attitudes, and expectations learned from significant others.

The study of sequences of behavior associated with drinking is just one aspect of the influence of sociocultural perspectives on alcohol studies. The fact that the frequency with which problems occur in association with drinking does not vary in direct proportion to quantity and frequency of absolute alcohol intake in a given society came to be accepted only in recent decades, on the basis of accumulated cross-cultural evidence. Sociocultural perspectives have also done much to explain why the French view alcoholism as a problem of medical pathology, while the Finns consider it a public safety issue, "the Irish sickness" is viewed as alcoholic psychosis, and so forth.

The recognition and analysis of subcultural variants within

large societies have resulted in general propositions that have gained broad acceptance, such as propositions that ritual and symbolism have important effects on drinking habits, that cultural ambivalence toward alcohol use can pose hazards for individuals, and that correlates of typically "convivial," "utilitarian," and other meanings are attached to alcohol.

What Weaknesses Are Evident?

In my role as critical commentator on the sociocultural model, it seems important that I offer specific and pointed criticisms about past and present efforts before I turn to prospects for future contributions.

I have already briefly addressed the issue of methods. The point I would emphasize is that rigor is only one measure of the usefulness of tools for research—other measures may often be more important for particular kinds of understanding in sociocultural studies.

But data gathering is only a first step in attempting to understand human behavior. Many of the difficulties that occur when investigators try to use these data are far removed from the exigencies of the field situation.

One fundamental and recurrent weakness in sociocultural writings on alcohol is the tendency to make comparisons among noncomparable categories—the fallacy of comparing apples with oranges. This occurs in a variety of contexts. For the purposes of our discussion, let's consider the confusion that derives from different kinds of norms, different kinds of cross-cultural studies, different kinds of ethnic groups, and different kinds of problems.

Drinking studies are by no means unusual in the fact that the term "norm" is used in vague, and sometimes varied, senses. In fact, this seems to be a shortcoming common to most studies that include discussions of norms.

Many authors use norm with no qualifier or other specification. In many such instances, context suggests that the meaning is quasi-statistical, referring to a pattern that predominates in a population's beliefs about what its members do. This first kind of norm (norm_1) refers to norms (or rules) of behavior—what is locally considered to be the normal or typical way of thinking and acting.

A second usage of norm without any qualifiers is also commonplace. In some instances, context suggests that the meaning is primarily regulatory, referring to the popular view of what people

ought to do. This second kind of norm (norm_2) refers to norms for behavior—to what is locally considered to be the good or right way of thinking and acting.

Still another kind of norm is occasionally reported by social scientists, and again it is usually context that provides the clue for meaning. The third kind of norm (norm_3) is again quasi-statistical, but it is primarily descriptive, referring to norms derived from behavior—to what a trained and relatively objective reporter discerns as the "real" way most members of the population actually do think and act.

It is common knowledge, among laymen as well as social scientists, that these three kinds of norms are not necessarily congruent. In fact, they often turn out to be grossly contradictory, with each differing markedly from the others. Unfortunately, although the term "norm" plays the title role in our conference for the next few days, I will be very much surprised if we don't find ourselves occasionally talking past each other by neglecting to distinguish the "normal norm" (norm_1) from the "normative norm" (norm_2), and both of those from the "modal norm" (norm_3). I lay no claim to having chosen the most appropriate labels, and I certainly share the widespread distaste for subscripts except when needed for special illustrative purposes, but I do feel strongly that authors must be reminded that they should more clearly specify their meaning for the word norm.

Another semantic pitfall in the sociocultural literature on alcohol lies in the multiple meanings given the term *cross-cultural studies*. Again, I suggest that we can distinguish at least three ways in which that term has been used in the social and behavioral sciences during the last 30 years.

The most prevalent kind of cross-cultural study is that in which a few populations that have different institutions are compared in terms of one or a few major aspects of culture (such as modernization, urbanization, political socialization, education, etc.). Economists, political scientists, and some others habitually use the term for comparative studies where the N of the sample is rarely larger than 6 and often only 2. The populations compared are often nation-states, or categories labeled by the investigators (regardless of whether such categories are meaningful in local terms). Examples are studies of "the role of entrepreneurs in the economic development of . . . [five nations]," "aspirations of slum-dwellers in . . . [two Latin American capitals]," and so forth. It is a tender trap that many in alcohol studies still find convenient for pedagogical purposes, citing variations between, say, France and Italy with respect to various indices of the epidemiology of

alcoholism. If then we link those differences with differences in drinking patterns in a superficial way, it is easy to imply the etiology is far simpler than any of us really believes. Or when exceptionally rich data are available allowing controlled comparison of particular patterns of belief and behavior from a couple of societies, we may be tempted to make unwarranted cross-cultural generalizations on the basis of a sample that is clearly inadequate.

A second kind of cross-cultural study is even more important in anthropology and has been used with unusual frequency and with extraordinary impact in studies on alcohol. This is the extensive holologic enterprise, in which the co-occurrence of particular traits, institutions, or other cultural items is statistically evaluated in a large and diverse sample of sociocultural systems, and statistically significant correlations are taken as clues suggestive of culturally, socially, or individually significant associations. The best known example of this is probably Horton's unequivocal assertion that "... *the primary function of alcoholic beverages in all societies is the reduction of anxiety*" (1943, p. 223). Horton's study, based on material from the 56 societies for which data were then adequate concerning alcohol use and various indices of anxiety, is still cited for its pioneering methodology long after many of its conclusions have been rejected. It is striking that in a discipline where restudies are extremely rare, the same data were reworked by Field, who focused on elements of social organization and concluded that "... drunkenness in primitive societies is determined less by the level of fear in a society than by the absence of corporate kin groups with stability, permanence, formal structure, and well-defined functions" (1962, p. 58). Larger samples and more rigorous methodology were used in a study by Barry and associates (1965). This study correlated several aspects of alcohol use with various aspects of child training and adult role-expectations, emphasizing the functions of drinking as a reaction to the conflict between dependence and independence. A contrasting conclusion, in which McClelland and his associates (1972) asserted that men drink primarily in order to feel powerful, included some fragmentary evidence from cross-cultural correlations of folkloric themes and drinking patterns.

Readers who focus on the conclusions, without following the meticulous explication of method in such studies, often misinterpret statements of association as being affirmations of causality, and mistake heuristic devices for causal entities. Although mistaking correlations for cause-effect linkages is a pitfall only for beginning students, the tendency to transform heuristic indices into causes occurs in some serious writings by professional schol-

ars. Illustrative of this fallacy is the frequency with which social anthropologists writing about drinking patterns in the 1950s and 1960s took pains to point out that warfare, witchcraft, and/or famine were of little importance in the population under study—addressing those factors that Horton (1943) had selected, for purely heuristic purposes, as *indices* of anxiety, *not* as particularly important *types* of anxiety. Similarly, it is unfortunate that the conflict over dependency that is so important in the work of Barry and associates (1965) is often foreshortened in discussion in such a way that “dependency” conveys a meaning very different from what the study was addressing.

A third, and very different, usage of the term “cross-cultural studies” is that in which some sociologists, psychologists, and others have paid attention to specific features that vary among self-defined subpopulations within a single nation-state where cultural pluralism is recognized and vital. In such studies, the code word has long been “ethnic” (although this term has recently become the focus of considerable controversy and attempts at greater specification). Equally vague is the term “subcultures.” Illustrative of this kind of cross-cultural perspective are those studies, usually based on questionnaire surveys, that compare, for example, Jews, Irish, and ascetic Protestants, or Mormons, Jews, Chinese-Americans, Irish-Americans, and so forth.

There is no doubt that such work provided valuable insights at a time when the importance of attitudes and values was largely ignored by physiologists, biochemists, and others who were trying to understand the influence of alcohol on human behavior without reference to sociocultural variation. The principle that such differences among categories of people do exist, and that they play some important role (in combination with other factors) in shaping patterns of drinking and drunkenness, seems to have been generally accepted for several years, and further replication seems unlikely to provide new insights. In fact, it is perhaps ironic that social scientists may not be less comfortable with such studies than are specialists in other fields!

In connection with this final kind of cross-cultural study, I am concerned about yet another class of comparisons among non-comparable categories—different kinds of “ethnic groups.” Categories of people such as those compared under the rubric of “ethnic groups” are often not really meaningful units in any sociocultural sense. Differentiation is not based on any uniform criterion; for example, “Jews” may be distinguished in terms of broad religious affiliation, where “Irish” may be labeled on the basis of father’s birthplace, and “Mormon” is a narrowly sectarian

affiliation. Beyond that, customs observed only by limited groups of Orthodox Jews are sometimes cited as important in shaping the attitudes of the majority of Jews, who do not share those customs any more than do the Irish. On the basis of what we know about linguistic, dietary, and other striking cultural contrasts among regions of China, there seems little logic in treating "Chinese" as a unitary population in terms of alcohol use. Similar shortcomings could be cited for most of the other "ethnic groups" that are regularly referred to in such comparisons.

Quite apart from these logical inconsistencies, it is noteworthy that the growing interest in ethnic groups in anthropology today has little to do with patterns of belief and behavior that are popularly identified as characteristic or typical of members of such groups. It has been found that the ways people define and maintain social boundaries between or among self-identified categories are often far more important and revealing of sociocultural dynamics.

A final type of comparison among noncomparable categories that is disturbing in connection with the sociocultural model for alcohol studies is that of different kinds of "problems." Unlike the types discussed above, where the confusion is largely semantic, there appears in this type a significant selectivity of perception (or at least of concern) that distorts the worldwide sample of descriptions and analyses of drinking behavior. One of the most striking contrasts between the uses and meanings of alcohol as represented by social scientists in Western and in non-Western cultures is strong emphasis on problems in the former, and the relative absence of problems in the latter. The World Health Organization, after groping for years to come up with a meaningful cross-national definition of "alcoholics," finally characterized them as "excessive drinkers whose dependence upon alcohol has attained such a degree that it shows a noticeable mental disturbance or an interference with their bodily or mental health, their interpersonal relations, and their smooth social and economic functioning" (1952, pp. 15-16). Citing WHO, anthropologists and sociologists have tended to assert that alcoholism is absent in particular populations on the ground that no member evinced any obvious problems, whether in terms of social relations, psychological adjustment, economics, or gross physical pathology.

I have no doubt that the frequency with which drinking leads to, or is associated with, anything that the drinker might consider a problem is much lower in tribal and peasant societies than it is in complex urban societies, but it may be fruitful here to shift

the focus from drinkers to researchers. In this connection, it is striking—and may even be significant—that social scientists have tended to look for very different kinds of problems in these different contexts. In dealing with drinkers who were reared in a literate ambience, there is a prevailing disposition to look for problems that might result *from* drinking. In dealing with drinkers from a nonliterate background, by contrast, there is a strong inclination to search instead for problems that might result *in* drinking. In short, drinking is conceived as *causing* problems for the economically and politically dominant segment of the world's population, but as *relieving* (or at least resulting from) problems for those who are dominated. I hesitate to venture a psychological or materialistic interpretation of that fundamental difference in views about etiology, but there may be a heavy load of cognition as well as affect there.

A final major weakness that pervades the literature in which the sociocultural model is applied to alcohol and human behavior is the implication of cultural homogeneity. By this I mean that most of the studies, by emphasizing norms (of whatever sort), offer only highly generalized statements about patterns of belief and behavior. There is rarely any indication of the range of variation among individuals with respect to how often, how deliberately, or how closely they conform to the patterns in question, nor is mention made of any standard divergencies from the norm that might themselves be other patterns characterizing acceptable alternatives in the nature of subnorms for various categories (such as women, youth, and so forth). It is true that there are some societies in which conformity is not only the normative norm, but also the normal norm, and even the real norm. But experience suggests that, more often than not, even a small and relatively homogeneous population will show a distinct range of variation among individuals with respect to beliefs and action patterns, whether in connection with drinking or with any other aspect of culture.

Although it is convenient in terms of interpretation and exposition to deal with typical characteristics, rather than attempting to deal with the complex diversity that really distinguishes individuals, such a convention obscures important portions of reality. This is particularly pertinent when we are dealing with behaviors that we know are associated with grave problems for a small segment of the population who drink in what we ourselves consider an abnormal manner. Let's emphasize the statistical rather than the evaluative sense of that word "abnormal"—the point is that our own immense concern over alcohol abuse and alcoholism in the United States might easily be overlooked by a Martian eth-

nographer if he paid attention only to the dominant pattern (that norm of "social drinking") and ignored the much less frequent and less visible variations that loom so large in our social and scientific consciousness.

In dwelling so long on the weaknesses of the sociocultural model, I do not intend to discredit it, but only to offer constructive criticism. It may help explain why so little of note has appeared in recent years, and it may prompt some changes in approach that could broaden or deepen our understanding of alcohol and human behavior.

What Are Some Prospects for New Contributions?

Far from being resigned to the idea that the sociocultural model has outlived its usefulness, I am convinced that it still holds great promise not only for understanding the forms and meanings of drinking in other societies but also for dealing with alcohol abuse and alcoholism in our own. In order to search out these valuable insights, however, a variety of different tactics should be tried. Some of these may be new, and others may involve expanding, refocusing, or subtly adapting approaches that have already been undertaken but have not yet had a major impact in the literature on alcohol studies.

- *We should pay special attention to alcohol in a sociocultural context.* In one sense, that may sound specious, because it is what this whole conference is about. But in another sense, it is not at all trivial in view of the fact that the vast bulk of the literature on the sociocultural model to date has been a by-product, derived almost coincidentally from investigations in which the researcher was paying far more attention to other things. In the last few years, it has been encouraging to see some people setting out with the specific aim of studying alcohol and human behavior in the field, and a few programs have been established where relevant research skills are being taught. The quality and quantity of pertinent data that are compiled when the subject is focal to one's research should be exponentially better than the results of even the most conscientious field-worker's serendipitous gleanings from field notes that are tangential to the subject.
- *We cannot afford to ignore variation within populations.* In much the same way that other specialists in alcohol studies managed too long to ignore variations *among* human populations,

too many sociocultural studies are too narrow inasmuch as they imply cultural homogeneity. Among weaknesses of the sociocultural model, I have already discussed the unrealistically uniform description of patterns of belief and behavior that results no matter which kind of norm is being described. While it may be true that few Orthodox Jews become alcoholics, it would be fascinating (and perhaps theoretically and therapeutically significant) to know in detail about some who did. While most Maya men frequently get drunk in a highly stylized manner, there are some who don't, just as a few Maya women drink, although normatively they shouldn't. What is the range of such deviations from the mode, or from the ideal? What is the frequency with which individuals elect such deviant patterns? On what bases do they make such decisions?

This concern with detailing variation, rather than single patterns, is at least equally important when our focus of reference shifts from individuals to social categories. We know, for example, that in most societies men are expected to drink more than women, but we know almost nothing about why this is so. Similarly, we know that young people are usually permitted to drink less than adults, but we know little about other gradations of age that may be important with respect to drinking among various populations, or about what criteria (other than chronological age) may be relevant in assigning individuals to such categories.

Different subgroups within any population are subject to different rules and expectations; they interact in different ways with institutions and individuals both within their own society and in other sociocultural systems. In many societies, drunkenness is the prerogative of a few persons of exceptionally high status; in others, the opposite holds true, and rarely do we have any insight into the rationale for such patterns, or any understanding of how frequently violations of the rules occur or how they are treated when they do happen. Among many North American Indian tribes today, heavy drinking is very closely linked to anomie, social isolation, and low socioeconomic status; among the Navaho, by contrast, it tends to be associated with security, combined power and prestige, and high socioeconomic status. An appreciation of different roles within the social structures is indispensable if we are to measure and evaluate such variations. In a community where many males are in close and sustained contact with alien drinking patterns, whereas most females continue in the insulated context of the home, what changes may occur? And, by the same token, what changes might we find in a community where more women interact regularly with outsiders, while most men remain

occupied with traditional tasks? In addressing such questions it is important to consider not only exposure to alternatives, but also values and attitudes about subgroup behavior, changing opportunities (and threats) about status, and a variety of other factors, some of which pertain to most members of a social category and some of which may be subject to large-scale idiosyncratic variation. It is in this connection that the tools of social-psychological investigation, which are being progressively refined and strengthened, might fruitfully be incorporated to complement the research methods that have heretofore predominated in sociocultural studies of drinking.

• *We should pay more attention to the processes by which people learn about drinking, drunkenness, and drunken behavior.* To say that patterns of belief and behavior are learned rather than inherited, and that they are shared (at least with others in a given category) within a population, throws little light on the systems of cognition that are involved, the patterns of reward and punishment that are used, the affective context of crucial experiences, the importance of role models, or a multitude of other factors that may be influential in shaping an individual's decision to conform or to reject particular norms among those offered. It is apparent that in most societies, strong views about drinking are inculcated long before the child actually participates in drinking to any socially significant degree. We know little about the relative importance of adults and peers (although this may be significant in our own society); we have little understanding of the relative impact of example and exhortation or of different kinds of reinforcements; and almost no attention has been paid to the theoretically crucial discontinuity that often occurs as young people are permitted, expected, or required to adopt adult rules that often contrast diametrically with those to which they have been subject. This discontinuity may be an important component of the ambivalence toward alcohol that seems to be associated with drinking problems in many cultures, and it certainly is of immediate relevance in any attempt at prevention or education about alcohol abuse and alcoholism.

• *We should try to learn how others view alcohol and human behavior.* At one level, this has been the major success of the sociocultural model, letting us know the range of human variation in uses and concerns about alcohol. At another level, most such efforts have been superficial compared with the kind of inside view that can be achieved through ethnoscienitific methods or other more focused research. Most of the sociocultural interpretations have been couched in terms that are familiar to social

scientists in our own society, and there is little assurance that members of the groups in question would accept such interpretations. More to the point, it is probable that some distinctions that are significant to the populations under study have simply been overlooked.

This probability has already been mentioned in connection with the expectation that research with a focus on alcohol will produce more insights into the subgroups within which different norms prevail, the reasons why individuals conform or not, the effects of conformity or nonconformity, and so forth. It may also be helpful to learn the folk taxonomy of drunkenness, in which degree of intoxication (as indicated by cues which are meaningful markers to members of the group) may relate to expected forms of behavior or to degrees of freedom in terms of rule-breaking, and so forth.

Similarly, the labeling process as it pertains to kinds of drinkers (or kinds of drunken comportment, etc.) may not only be revealing in terms of social dynamics in a given context—it may also provide insights into the subjective definition of various kinds of problems, and it may yield a sort of folk symptomatology of alcoholism which would in turn be a powerful tool for anyone who wanted to mount effective programs of education, prevention, or treatment of alcoholics in various populations.

- *We should try to learn about the adaptive strategies of individual drinkers in various sociocultural contexts.* In shifting from homogeneous global descriptions to analyses that identify not only variation among segments of the population but also variation among individuals, special attention should be paid to the processes by which people identify what alternatives are available to them, and then select among those. In short, we should try to learn about decision making as it applies, for example, to a man's declining to accept a prestigious and otherwise rewarding position of responsibility in order that his drinking habits not be jeopardized. Or there might be valuable meaning in the ways in which those who drink in an abnormal manner try to hide, deny, or excuse the fact.

- *The drinker should always be considered within a context of interpersonal relations.* Just as we have difficulty understanding the act of drinking unless the context of values, symbols, and other meanings is taken into consideration, it is also narrow and misleading if we try to understand an individual drinker unless the network of social relationships within which the drinker lives is taken into account. The role(s) of significant others may be crucial in terms of holding expectations, manipulating rewards and punishments, serving as a model, and so forth. Often the process

of labeling is as much relational as it is intrasubjective, so that the appropriateness of an individual's eligibility to participate in and be judged by the norms of a particular subcategory within the population may be regulated less by choice or achievement than by ascription.

There are some interesting hints in our own society that the size of an individual's network of primary social relations decreases as the severity of alcoholism increases; perhaps this is a corollary of the enormous importance that attaches to the fellowship of Alcoholics Anonymous as providing a new social support system for one who has been progressively alienated from normal linkages. A network approach could contribute not only to our understanding of how and why an alcoholic becomes isolated, but also to the wide and varied ways in which drinking impinges on people other than the drinker. The ways in which friends, kinsmen, co-workers, and others interpret and react to an individual's behavior often constitute a significant portion of the psychological and sociocultural ambience, and can perhaps be used to reinforce or to extinguish certain patterns.

- *We cannot afford to ignore biology.* Although it was a long, uphill struggle to achieve a measure of academic respectability for the sociocultural model, it would be an empty victory if we were to ignore other valid and valuable perspectives. Having made the point that people are much more than biological organisms, it may now be important to look more closely at what roles the body does play in shaping the drinking patterns that characterize various populations. In an overreaction against emotionally based racism, social scientists may be overlooking metabolic, enzymatic, and other differences that set significant limits on how individuals (and groups who share such physiological characteristics) can drink. The traditional firewater myth that some racial groups are particularly susceptible to alcohol, and that a small quantity makes them dangerous, has been generally discredited on the basis of historical and ethnographic evidence, but "the new firewater myth" that North American Indians are peculiarly immune to alcoholism is not supported by the data either (Leland, 1976). Increasing experimental evidence suggests that there may be significant differences in the effects that alcohol has on members of various populations; the results reported by different experimenters are by no means consistent, and both the techniques of testing and the nature of samples have been criticized. Nevertheless, those who are interested in the range of human variation should have a special concern for helping to distinguish between biochemical and sociocultural factors.

- *There is a real need for longitudinal studies of drinking if we*

are to address almost any of the important questions about the interaction of alcohol and human behavior. Although this shortcoming is probably felt at least as acutely by those who are primarily concerned with biomedical aspects of drinking, it seems as if those interested in sociocultural perspectives have been more outspoken in reiterating the importance of long-term research. Only in this manner would it be possible to assess with any confidence the relation between what children learn about drinking and how they drink as adults. The links between modes of socialization on the one hand, and drinking norms on the other, seem plausible and even compelling, but might be strengthened and sharpened if we were able to follow the way they operate in the life history of individuals. Of course, exceptions to the rule would be at least as revealing, in different ways, and the interplay of societal, cultural, and individual pressures and opportunities would have relevance far beyond alcohol studies. Among individuals who develop various kinds of problems, a longitudinal perspective holds the only realistic promise of our being able to understand how such problems originate, how various means of coping may be selected, tried, rejected, modified, etc., how the individual and members of the social network assess and react to various coping mechanisms and their outcomes, and so forth.

It may very well be futile to search for any kind of standard developmental sequence or progression of phases of alcohol addiction, as some have tried to do on the basis of retrospective data (which are notoriously unreliable). But it remains an empirical question whether significant patterns may recur in different types of drinking histories. It would be an important finding if we were able to document, for example, the way(s) in which many drinkers seem to experiment with very different styles of drinking at different times, and some appear to move in and out of the problem drinker category instead of progressively accruing more problems, as is often suggested in the literature.

Related concerns that can best be studied in a temporal context include the developmental cycle of alcoholic families or other kinds of reciprocal relationships that are affected by the drinking of one or more members. In this connection, the processes of labeling and of learning may again be clarified because causes and effects of other events and processes can only vaguely be reconstructed by even the most articulate and conscientious participants after the fact.

In all of these prospects for new contributions—paying more attention to variation *within* populations, focusing more on how people learn what they do about alcohol, trying to understand in

more detail what people do know and feel about drinking, attempting to analyze the adaptive strategies of individuals with respect to alcohol, incorporating the drinkers' social networks in our assessments of their situations, taking into consideration biochemical differences that may affect reactions to alcohol, and striving for greater time-depth in all our studies—the relevance is far broader than merely increasing our confidence in the sociocultural model. Our common concern with better understanding of why people drink as they do—or, equally, why they decline to drink—and how that relates to other aspects of their lives (whether in positive and facilitating ways or in negative and problematic ways) demands that many different perspectives and methodologies be brought to bear. One of the special fascinations of alcohol studies is also one of its greatest frustrations—no one approach can provide more than a fragmentary glimpse of the complex reality. Nevertheless, despite all its weaknesses, the sociocultural model has contributed in significant ways to our increasing understanding, and it seems likely that, in combination with other approaches, it may continue to offer important insights that cannot be gained from biomedical or other more rigorous scientific perspectives.

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Discussion of Paper by Heath

Robert Straus

Dwight Heath's thorough, thoughtful, scholarly, and challenging review poses a serious dilemma from the point of view of a discussant. Generally, a discussant is expected to find some significant points with which he can disagree and which he can discuss critically. However, I find that I agree with nearly everything Dwight Heath has said, and there is little I can criticize. A discussant can choose to expand agreement into a series of polite "me too-isms," or can try to extend some of the points raised—but even this will be difficult because as I read Heath's paper I found that he systematically covered almost all my own pet biases. Nevertheless, there are some points I would like both to commend and to extend.

1. *Social Science Methodologies.* Heath's paper begins by addressing the methodologies of the social sciences. He makes a strong case for the importance of case-study methodology and of qualitative data as neither better nor worse than survey methodology and quantitative data in providing conceptual insights.

I believe that this issue is particularly worthy of emphasis because many social scientists appear to have forsaken the observational and qualificational roots of their discipline in favor of the mathematicization of data no matter how irrelevant. I am deeply distressed by the number of young sociologists who are superbly trained in the mathematical analysis of survey data but who have never had the experience of generating questions. Some young Ph.D.'s who have never themselves conducted an interview undertake to hire and train interviewers. Doctoral degrees have been awarded to individuals who have never designed or pretested a questionnaire, for dissertations based on data collected by others. Far too many questionnaires are developed by armchair investigators who have never met or talked with any representatives of their subject population in order to learn what the meaningful questions are and how they can be phrased so that they mean the same thing to those who answer them as they do to those who ask them.

I firmly believe that training and experience in case-study methodology should be considered an essential part of training for survey methodology. All the quantitative rigor one can muster will not provide fruitful insights or useful findings if the data being analyzed are not based on relevant and significant questions that

are phrased in meaningful language. In short, I believe that whether case-study, experimental, or survey techniques are used, all social scientists should have experience in deriving qualitative data through personal contact with human subjects and that research subjects should be included in the determination of "What are the questions?" as well as "What are the answers?"

2. *The Case for Longitudinal Prospective Research.* Heath addresses the need for longitudinal studies of drinking and particularly for prospective studies that trace the natural history of drinking careers as they unfold. This need applies to scientific inquiry generally and indeed to many avenues of scholarly activity.

The paucity of longitudinal, prospective research is not due to inherent methodological problems. (Indeed, retrospective research, which must rely on selective memory and on perceptions distorted by intervening experiences, has many more methodological difficulties.) Instead, longitudinal research is precluded by prevailing policies on financing research and on recognizing and rewarding scholarly achievement.

Specifically, for many years it has been almost impossible for investigators to obtain research support for periods of more than 5 years; in recent years more and more awards are limited to 3 years, particularly for young investigators. Furthermore, with research support frequently tied to agency-defined priorities that are subject to rapid change rather than to the priorities of basic scientific merit, investigators cannot always anticipate continuity of support for long-term research even when their findings are promising and their methodology sound. Thus there is a great temptation for research workers to plan studies that are sufficiently small in scope and short in duration to permit closure within their period of committed support. Those who do plan long-term programs of research do so with an increasing awareness that unless they can produce quick findings, they may be subject to the whims of shifting grant policy and priorities.

An even more serious deterrent to longitudinal research is the policy, vigorously advocated by the American Association of University Professors, whereby many universities now require that tenure for faculty members be earned within 6 years of full-time academic activity. Because qualifications for promotion and tenure are nearly always heavily weighed by the number of publications that reflect scholarly and scientific productivity, there is enormous pressure on young scientists to forego longitudinal research or other time-consuming investigative activity in favor of studies that can produce quick and easily publishable findings. Although early tenure may appear to offer the kind of security

that would encourage participation in long-range research, ironically policies that make academic survival depend on the number of studies completed early in one's career engender attitudes and habits that often conflict with long-term commitment to significant research problems.

Clearly, in the area of alcohol studies as in scientific activities generally, we need to encourage promising young investigators to think and plan in terms of career-long research. But policies for research support and policies for university promotion and tenure must be revised if we are to reward rather than punish the kind of career commitment that can stimulate the design of longitudinal, prospective studies of critical questions. Although they seem to have fallen from favor in Washington, career research grants can provide one incentive for meeting this objective.

3. *Adequacy of the Unidisciplinary Conceptual Model.* Dwight Heath has appropriately questioned the adequacy of unidisciplinary conceptual models for the study of drinking behavior and alcohol problems. The disciplines we usually call the social and behavioral sciences supply only fragmentary pieces of the conceptual insight and empirical knowledge necessary for the satisfactory study of human behavior. Drinking behavior and the problems of alcohol provide unusually vivid examples of the need for holistic or unifying models that facilitate examining the fundamental interactions between structural, functional, genetic, and chemical bases of what we usually call biological behavior; the psychological, social, and cultural bases of individual and group behavior; and the impact on behavior of environment, space, and time. For much of the last century, in response to an incredible spiral of expanding knowledge, especially in the biological sciences, we have been moving toward a greater and greater concentration of knowledge around disciplines and subdisciplines that have enabled investigators to acquire exquisite expertise in the theory and technology of their ever-narrowing foci of concern. Yet as we have expanded our knowledge and refined our focus, we have been moving toward a point of development in the pursuit of knowledge at which traditional disciplinary boundaries have become meaningless and obsolete. Today, in almost every field of inquiry, the questions with which we are dealing cut across disciplinary lines and encompass the traditional territory of several disciplines. Specialists during the latter part of the 19th and first half of the 20th centuries were working within information storage and retrieval systems that relied heavily on human memory and printed language. They limited their perspective to those fragmented spheres of expertise that could be encompassed within

available information systems, and, appropriately for their time, they chose to strive for complete knowledge in a limited sphere of inquiry rather than superficiality and breadth. With today's methods of information retrieval and synthesis, however, superspecialists can no longer justify ignoring the findings of related disciplines that interact with and add meaning to their own.

In the alcohol field, an example of the need for multidisciplinary perspective is found in several studies of biochemical and pharmacological factors that have been conducted in controlled human experimental laboratories in which subjects lived for extended periods of time. Although several such studies have been designed and conducted with great care and inventiveness, they have shared one important sociocultural limitation. In order to find volunteer human subjects who were willing to live under conditions of incarceration, these studies had to rely on homeless men with long-term histories of institutional living in jails, public shelters and on skid row. Because in the laboratory setting these subjects do not consistently display a compulsive need for alcohol, their behavior has been interpreted as a basis for rejecting the controversial concepts of "loss of control" and "craving" in alcoholism. The interpretation placed on these findings by biologically oriented investigators might well have been quite different had they considered as well the findings of available social science studies on drinking patterns of the homeless, institutionally dependent population that had already identified a pattern of non-addictive pathological drinking strikingly similar to the controlled drinking, noncraving phenomenon seen in the laboratory settings. In the perspective of the social science findings, the biological findings appear to be more characteristic of the drinking behavior of a group of subjects and their response to the research setting than to the alcoholic population in general.

4. *Sociocultural Factors in Dependency on Alcohol.* Turning more specifically to drinking behavior, I would like to expand somewhat on Dwight Heath's discussion of a sociocultural model for conceptualizing drinking customs and problem drinking. Here I agree with Heath that social scientists must be much more aware of the significance of *individual differences* in human response to alcohol. There are significant differences between individuals in sensitivity to alcohol, just as in sensitivity to caffeine, aspirin, antibiotics, and nearly every other chemical substance. There are also variations in response to alcohol under many different conditions, such as time of day, level of fatigue, content of stomach, prevailing mood, environmental stimuli, simultaneous use of other drugs.

Drinking problems generally occur only when individuals drink more alcohol than they can handle. For individuals who are hypersensitive to alcohol, even one drink may be too much. For those who tend to be hyposensitive, several drinks may not be too much.

In addition to the nonspecific biological factors that influence sensitivity, there are factors of personality that influence the function or special meaning that alcohol has for particular individuals. In general, I wonder if we cannot say that the more meaningful alcohol is to an individual in terms of its mood-modifying functions, the greater the danger that drinking can become dysfunctional in terms of a person seeking to drink too much.

If we can accept the concept of considerable biological and psychological variation in how much alcohol can be consumed before an individual is drinking too much, then I believe we can identify a major factor in the etiology of problem drinking and of dependence on alcohol as the tendency for the cultural norms and reference group sanctions of many societies to ignore variations in individual drinking capacity and to impose pressures on all persons in a drinking setting to drink in accordance with the comfortable capacity of those who are in the middle or even the upper range of sensitivity.

Thus, the member of a factory carpool who can comfortably drink only one beer will be drinking too much when, to be a good fellow, he repeatedly has two or three beers on the way home from work. The individual who cannot comfortably drink at noon, drinks too much when he regularly participates in a round of martinis for lunch. The salesman who has hyper- or mid-range sensitivity drinks too much if he tries to keep up with every hyposensitive customer. There are countless examples of social pressures to drink too much, including the pressure on some women who feel they must demonstrate sexual equality by drinking like men.

Additional sociocultural pressures toward conformity in drinking appear in an increasing number of social situations in which drinking is defined as appropriate or expected behavior, and the nondrinker or the one-drinker may be made to feel that he or she is inadequate or impolite, violating amenities, and spoiling the enjoyment of others. Many of these situations involve leisure time, but some drinking requirements are tied to situations involving major social responsibilities as in diplomacy, government or military leadership, and corporate management.

Sociocultural pressures toward conformity in drinking too much are supported by the frequency with which heavy drinking

is depicted as associated with success in fiction and the modern mass media. Over and over again our population is exposed to the image of beautiful women and handsome men consuming what for most people would be too much alcohol and then proceeding to be successful in outwitting their adversaries, driving their cars at high speed in heavy traffic or on mountain roads, catching a criminal, or making magnificent love.

I suggest that the various sociocultural factors just mentioned create a potential for a kind of social dependence on alcohol. I believe that many of today's problem drinkers are people who begin drinking too much not in response to a deepfelt need for alcohol, but because they think they should drink in order to meet social expectations, to avoid social censure, or successfully to complete a social task. Some of the people who repeatedly drink too much because of social dependency may eventually develop a psychological or inner need for too much alcohol; others may change their lifestyles in favor of social situations that don't require too much drinking; still others may be able to adjust their values and adopt an independent and personally safe drinking posture.

Even among the socially dependent drinkers who develop an inner sense of need for alcohol, there are some whose psychological dependence may be specific to certain social situations, while others develop the more generalized need for alcohol that is associated with the term "alcoholism."

I suggest that we differentiate among people who have a social dependence on alcohol, those who have developed a psychological dependence that is specific to particular social situations, and those whose psychological dependence has become generalized (with or without physical dependence). I believe these categories are significant in understanding the etiology, course, and distribution of alcohol problems and that they are meaningful in developing strategies of prevention, treatment, and social policy. They may even throw some light on the processes of remission, the movement of some people in and out of problem drinking, and the current controversy over whether certain categories of problem drinkers can learn to use alcohol safely.

General Discussion

Griffith Edwards: Here are a psychiatrist's comments on a sociologist's comment on an anthropological paper.

People drink for a variety of reasons and in response to a variety of cues. Any one person is susceptible to a wide range of cues, and these cues will cumulate and interact with hierarchies of importance that will fluctuate over the course of time. Some cues come from the environment—a person may drink because he is with friends, because there is a fiesta, or because it is time to receive the sacraments. These are obviously social cues. But there are other types of cues, such as mood states, that may suggest drinking. Anxiety and depression may serve as spurious grounds or cues for drinking.

Other cues have been ignored in our discussions because they are difficult to grapple with. There are cues related to the withdrawal experience—not only the gross trembling, but also the minor discomforts of the affective disturbance that may come from withdrawal. Only an unlikely model would propose that cues ever exist in splendid isolation. The way we interpret cues depends on our mood state, our basic personality, our culture, our childhood experiences, and the immediate setting.

We need a dimensional model of dependence that can take account of interactions. Potentially, the idea of social dependence is misleading if it accidentally suggests a concreteness to what is really an interreactive field.

Harvey Brenner: Serious problems are encountered in research designs, even where the materials occurred over a period of time, which should actually afford us some advantage. The example given is this: we observe that with the increasing severity of alcohol problems, the number of associations, friends, or whatever the person has diminishes. Now, which is causing which? Is it that the alcohol problem is bringing about the loss of friends? Or is the loss of friends bringing about the alcohol problem? Or are we talking about regressive problems, the feedback problem? Which one produces the other independently of the first, and in fact where does it all originate? This kind of approach, while useful as a middle point, does not tell us anything about the beginning or where things finally wind up. The point of it is that under the best conditions this modality of study is highly ecological in character and tells very little about the internal cultural relations of which we would like to know if we are to make any sense out of the true interactions among the phenomena.

Longitudinal studies do not help us at all. Once again we are not able to separate some attributes of individuals, mainly their drinking problems per se, from other attributes of the same individual that occur simultaneously, such as the loss of their friends, their relationship with their families, their work. We cannot segregate those factors because they occur simultaneously, even under the best conditions with longitudinal studies. There are resolutions to these problems that require much more complex and realistic models. They are the ones anthropologists and sociologists are used to examining when they go out in the field and see the "I" correlations—the relationships of what people do with other people. And what ultimately is required there are much more highly sophisticated facts, much more highly sophisticated mathematical treatments, and at the same time much more meaningful data. The longitudinal material, the network studies, are required, but at the same time they will not be thoroughly effective if we don't have the conceptual and mathematical tools for analysis.

As to the longitudinal study as a precise solution to a vast majority of our problems, particularly those that influence policy area—I think we may be mistaken here because in policy-oriented research we require some rather substantial differences among populations, or political units, that give us great differences in policies. In other words, differences in policies must be prevalent among the populations so that we can compare those populations according to the policy for a given population. Well, following multiple populations under differential-condition policy will give us that. So it is not just a question of the expenditure, which would be quite extraordinary both in human effort and career development alone, or of what has been clearly called a single government investment—it will probably require something on a multistate or multinational order to make sense for the important policy-relevant set of issues.

Related to this, and related to the earlier issues, is the repeated discussion of multidisciplinary efforts, which is quite direct. I believe it is so obvious, in fact, we usually forget it. As Ed Murrow said, "The complex we know in time; the obvious takes longer." And we're getting around to figuring out that dealing with reality problems requires multidisciplinary approaches. In doing so, however, the interactions are so severe, and there are so many variables that require consideration, that once again we require very sophisticated statistical tools in order to put together packages of analysis incorporating different levels of analysis and different disciplines as we now understand them.

Finally, I am most intrigued with Dr. Straus' conception of

social dependence as distinguished from—as opposed to—something you might call individual psychological endeavor. I think this is a very, very important idea—important because not much has been heard about it as an idea before. However, again, it is one of those very obvious things—obvious to the point that we generally forget about it. It is so obvious that it is hard for me as a social scientist to imagine anything else but a social dependence. How, indeed, this morning shall we understand why it is that people acquire the preference for alcohol, which they acquire if not through social interactions, through socialization systems. In which case, how is it at all possible to discriminate the individual from the social setting?

Richard Jessor: I want to comment both on the issue of norms and on the issue of method in longitudinal studies. It is important to see the issue of norms in a problematic light; the problematic relationship between norms and behavior is rather critical. It imposes on us, as Dwight was pointing out in his paper, a recognition that the relationship between whatever the norms are and the kind of behavior that occurs in a normative context is not singular.

Dwight talks about various kinds of norms, and I want to support that perspective. But I also want to raise other sources of the problematic relationship between norms and behavior. The ways in which norms influence behavior obviously depend upon non-normative factors as well. They depend upon personality factors and upon various other kinds of sociocultural factors, and I think the hope for normative explanations of behavior probably resides ultimately in some larger compass that deals with those factors that affect the way in which norms operate as well as with the norms themselves.

The other issue has to do with longitudinal studies, and I am very much in agreement with Dr. Brenner's comments. I just want to make a couple of points about longitudinal research. The first thing I think is important is that it is not necessary to think of longitudinal studies as a cradle-to-the-grave kind of approach. The recent longitudinal methodologies are much more manageable than the older ones. They refer to very short time periods. They may be time periods of just a few years in length, but the designs are set up in such a way that one has multiple panels going at the same time. This makes it possible to bridge a large time interval by short overlapping longitudinal studies. I think this makes the whole prospect of time-extended work much more manageable than the original approach of starting in infancy and working on from there—a time expanse that not many of us have the patience for or are likely to get reinforcements for.

The second point about longitudinal studies is not often given its due. Longitudinal studies have a contribution to make in description alone. We usually turn to longitudinal studies to get some sense of the causal structure of phenomena, but I think there is a marked absence in behavioral sciences of descriptive understanding—descriptive information about how people grow and develop and change. Having that kind of information ought to be useful in arriving ultimately at the causal structure.

The third thing I want to say is that even longitudinal studies are not an easy way to get hold of causality. One is usually dealing with time-extended sequences in which the events of interest are completely entangled. And while one can track the trajectory of change in those events, dealing with what causes what remains a very refractory problem. One way to resolve that difficulty may have to do with *onset* studies. One way to disentangle factors that lead to drinking consequences, or to drinking problem consequences, is to begin longitudinal studies *before drinking has begun or before problems associated with alcohol have developed*. This kind of onset study, which can be designed for almost any stage in the life course, makes it possible to observe factors that are operative prior to (antecedent to) the occurrence of the phenomena we have a particular interest in.

John Seeley: I have no question about the excellence of the papers or the stimulation of thinking both in the large and the small that they have provided. I would like to direct your attention, however, to the (at least to me) extreme oddity of the two contexts in which the discussion as a whole has been set. One context is, in a sense, microeconomic and is defined in terms of availability (which is itself very hard to define), and the other one, which we also heard about this morning, is sociocultural. Sociocultural analysis is an odd way for people who call themselves sociologists and social scientists to be satisfied that they have got anywhere near the root of the matter, or are indeed not dealing with minor or intermediate factors or whatever you would like to call them. When we are talking about "sociocultural" factors, I believe that generally we are talking about relatively small groups or groups that are defined as "ethnics." We are definitely not talking about the *general shape* of the society. The word "power" is notably absent; the word "interest" is notably neglected. It's kind of a nice little expanded familiaristic "How do we affect each other?" method of interpersonal and neutral analysis.

Apparently it would not be striking and shocking that the word "social" should have been joined with the word "cultural" in the bridge-term "sociocultural," thus directing attention away from

"society" and interest and power questions. "Culture" in this case doesn't mean these key matters or everything that is patterned into society: it just means the organization (and really the microorganization) of beliefs and behavior, broken down into rather small units of the action. The term would not direct our attention toward what happened to various people in relation to Mr. Nixon's debacle in the White House, nor to the whole social-problems structure in terms of which it is an open question whether—given the government's financing of research on one side and its profits from sales on the other, and its considerable interest in both—the sponsor has any interest in the *solution* of any problem as against the perpetuation of the tension between them (like a Royal Commission, set up in order to do nothing). All these things disappear in the term "sociocultural." The former tradition of the social sciences—that *whole* part of the social sciences in which you deal with the shape of the society as a whole and in terms of which minor beliefs (about alcohol or the desirability or frequency or infrequency of sex) could be related to the fundamental beliefs and interests and structures—that all disappears from sight with this kind of analysis. And valuable as "cultural" study is, and restricted as we are (but not accidentally), we have to consider its place and importance. (By the way, none of the things I heard of as difficulties or suggested remedies is there by accident.)

Let me just illustrate one point to make it clear. Someone suggested, I think with considerable insight, and I think it's really important, that the reduction of money put into education has probably led to "turfism," which militates against interdisciplinary studies, which militates against understanding. I'm willing to buy that. But it is no *accident*, at least in this country, that the funds for education were reduced. First, they were reduced in an economy move (and that implies a certain view of what is good for society as a whole). It was then left for Mr. Nixon and his successors, when they discovered that the effect of reducing money flowing into the universities was to blunt the force of criticism and to disenable travel by critical professors and students, to see more economy as a way to cut down the much-feared rioting in the street and other feared events that together made up the kind of paranoia that underlay Watergate. *Then* the reduction went into full force as a way of *taming* professors, in both senses—in their direct political stances and in their tendency to make dangerous inquiries into things that might actually *solve* social problems (one of which, ostensibly, we are here to discuss). So in reality the degree to which what we are doing here is a charade is something I will get into tomorrow.

Harvey Brenner: I think the next comment to make is on the issue of Dr. Griffith's comment on Dr. Straus' remarks on the social-dependence suggestion, which is that there be again some discrimination of this idea of social-sociocultural dependence, if you will—in normative behavior from, say, such things as internally generated mood phenomena that bring about some decline of need, desire, or whatever for things euphoric, including alcohol. Again, it is not for me to understand this discrimination; I can on an analytical level, but on a reality level, I cannot. It is quite clear immediately from a normative structure how individuals are permitted to react to stress phenomena, either externally or internally. Given pain situations, given difficulties in life, there are different subroots of society or conditions under which it is appropriate to deal with those tensions and stresses by the use of psychophenomena—by the use of alcohol in particular. For example, is it appropriate under conditions of bereavement in a society to drink a great deal, or a little bit, or how much? Is it appropriate to drink under conditions of diminished responsibility, etc., etc., etc.? The point being that I find it terribly difficult to discriminate the normative phenomenon of sociocultural as to what the society permits, encourages, or restricts, from the mental status that gives rise to some dishonor in utilizing the beverages or psychotropic material.

Robert Straus: I think Griffith Edwards and Harvey Brenner made some very important points. It was not my intention to discuss social dependency as an isolated or unidimensional phenomenon, but it may have sounded that way in my effort to stress how almost unbelievable it is to me that these kinds of motivations for drinking too much have been so neglected by the field. It was my intention to suggest that available evidence should lead us to look further at the probability that a very significant number of people who drink too much are drinking what is for them excessively in response to cues, or to clusters of cues, in which these social pressures that stem from outside the individual have a very high priority—not to the exclusion of other cues, but a very high priority. I think also that in the life histories of many people who drink too much we are going to find that, in the early stages particularly, social cues for many people have a much higher priority. They may reach a stage where they are giving higher priority to the meaningfulness of alcohol in terms of inner stress or tension reduction. But actually, social pressures themselves cause a large amount of the stress and tension associated with individuals drinking beyond their comfortable capacity. These individuals may then drink to alleviate the anxiety they would feel if they were not to

drink, because they wouldn't be one of the crowd, because they wouldn't be accepted, because they would be impolite, because they wouldn't be fulfilling amenities, or because they wouldn't be satisfactorily performing expected behavior in connection with their role as a diplomat, or as a salesman, or as a military officer who has to demonstrate that he can drink like a Marine.

Robin Room: Longitudinal studies turn out to be very difficult in terms of analytical procedures. Those questions of reliability that one can shove under the carpet in a single-shot survey are suddenly squared. I don't think anyone should underestimate the difficulty of longitudinal studies or overestimate the enlightenment that can be achieved from them. There are only some things that you can do with them. However, there are a lot of things that you can do with retrospective data that haven't been done. I think in a way the major benefit of longitudinal studies has been that it means that someone is paid to sit and look at their mistakes rather than move on to the next area. I think there is a very great virtue in people being paid to sit and stare in the face what they thought a couple of years ago.

On the concept of social dependence, I think that some notions of social dependence have been neglected for a couple of reasons. One is that social scientists in discussing norms, particularly in the alcohol area, have tended to make Hobbesian assumptions that norms act only to hold down behavior. Until very recently there was really very little recognition in the literature on norms that there might be norms that are not acting only to hold down behavior.

Certainly if you think of the institution of standing rounds, there are a number of social institutions that you can think of, well described in the observational literature, that are norms that enforce a behavior on the individual irrespective of what the individual himself may desire. Once you start looking at norms as acting to increase as well as hold down behavior with respect to drinking, then you have to face a concept something like social dependence if you are indeed to retain a concept of dependence at all. The concept of dependence, I think, really derives from clinical thought and is really one possible way to explain why people go on ramming their heads into the wall—why a behavior that seems intrinsically harmful, or at least to the observer seems irrational, continues. It seems to me that the explanation of that behavior must be sought at social levels as well as at psychological and physiological levels.

I certainly agree with Griff's point that the person who is socially dependent without being psychologically or physiologically

dependent may be rare, or the circumstance in which that occurs may be rare, but conceptually I think it is important to keep the dimensions separate. You can ask the question certainly concerning those who were using opiates heavily in Vietnam and then did not use them when they came back to the States, whether the controlling level in that situation was not a social dependence even if the physiological dependence was there.

The main point I wanted to make was about my attempts to grapple with placing the papers in the discussion this morning in the framework in which I understand the term "sociocultural model" to have arisen. Paul Whitehead and I were comparing notes here and as far as I can figure out, Blacker probably used it first as a label for one position in a specific debate. When we are contrasting a sociocultural model with a distribution-of-consumption model, implicitly we are talking about a particular debate that has been going on concerning how one intervenes in alcohol problems and how one prevents or minimizes alcohol problems. That debate really has been somewhat constrained in terms of the number of models that have been used. When one talks about sociocultural models, as I understand it from the literature, one is not usually making a statement about the model for describing behavior or a model for research. Because clearly, such a meaning does not denote a very specific or descriptive model. But the term has come into the alcohol literature with a specific kind of policy framework orientation.

That seems to me to hold some implications. What is usually used to refer to a model or a hypothesis was actually first stated by Ullman in full form, drawing on earlier sociological and anthropological work. It is a hypothesis about the rate of drinking problems that states that when norms are absent or contradictory, in a situation of ambivalence, you get an especially high rate of alcohol problems. It seems to me that if all one means by the sociocultural model is that if one is seriously interested in preventing behavior one must pay attention to sociocultural factors, then there are very few people around the room who would disagree with that, no matter what model they may adhere to. On the other hand, if you mean by it that it is the preferred strategy, that it is how we should go about tackling and preventing alcohol problems, then you might find considerable disagreement. And that is likely to be the area where in the end our debate will focus. Certainly the people who have stressed availability would not deny the importance of sociocultural factors; they would simply say these factors may be less manipulable, less easily turned to the prevention of drinking problems. It may be hard to get us all to

drink like Orthodox Jews. I think John Seeley is right in pointing out the narrowness of a debate that sets up these two models against each other and doesn't allow for any analysis of how one might go about diminishing drinking problems that looks at structural and economic factors.

Reginald Smart: I have wondered myself whether part of the interest in the sociocultural versus what might be called the distribution model might be looked at in terms of the sociology of knowledge. It is not an area I know intimately, but it has always struck me that the distribution approach is almost always accepted in countries with a strong socialist government, such as Scandinavia, and nearly everywhere in Canada. In these countries, there is a tendency for the government to be interested in prepaid health care plans, early pensions, family allowances, etc. The distribution model tends to be rather unpopular in places like the United States, which is less socialist and places more emphasis on individual freedom and community activities as opposed to, say, government activities. As I have talked to people about the data and the evidence, I think that sometimes there is a different set in the United States. When I talk to people from Scandinavia or Holland or other parts of Ontario, we seem to be approaching data set from a totally different point of view than that of people in the United States. We seem to be less worried about the implications for government control. The implications for government control are very considerable if you take a distribution approach. Take a sociocultural method and they're probably not so important. I wonder whether that doesn't have something to do with the whole debate. I just raise it as a question; I really don't know the answer.

Paul Whitehead: I was hoping that Dr. Heath could reply to the following point. Probably in the written presentation, but also in your verbal presentation this morning, you indicated a rather vast shopping list of questions that would be nice to have answers to. However, those questions weren't arranged in any kind of priority either in terms of what their theoretical relevance might be for assisting us in developing explanations or theories about alcoholism, or in terms of their programmatic relevance whether for the prevention or treatment of alcoholism. While various studies have any number of difficulties associated with them, the point is that there is a fairly sizable body of research that provides information relative to programmatic implications for prevention. I was wondering if perhaps it might not be possible to prioritize some of those questions. In other words, if understanding something about sociocultural backgrounds is to be important

say in terms of prevention or in terms of explanation, then what is the nearest set of questions to which one might like to have an answer, or what is the next set of questions that might produce a real payoff in terms of programs of prevention?

Harvey Brenner: One of our most important industries in the United States is advertising. This is the source generating most of what we eat, wear, drink, how we conduct our sex lives and our most private as well as public behaviors. The people who serve in these industries are graduates of the institutions in which we teach. They are among our most talented and sophisticated social researchers. They understand social phenomena extremely well, and they include psychiatric and psychological graduates of our behavioral sciences as well as the sociological wider-scale movements. It is these people who sell us nearly everything we buy and have a lot to do with the ideologies we utilize, the people we vote for, etc. This is all well known to us, but what is not apparently appreciated to any extent is that these are highly manipulable tools; we use them all the time. This is how the industry is arranged. This is how we normally bring forth the product of industry and burden the pockets and the bodies of the people who live in the society.

In socialist countries, it is done differently. It is done more programatically and without perhaps the same kind of market research, although this is changing because they have to do planning and sell shoes in the government store and all the rest of that. Now given all that, and given what appears to be, certainly over the last 20 or 30 years, a very high level of true manipulability of public attitudes toward better ways of life, it seems to me that the point of attack perhaps—if this is what Dr. Seeley was referring to—the most likely one does not lie within the legal sphere at all, but lies with the same system that brings to bear the large-scale social change in our way of life that is communicated through the mass media. We have many examples in the health sciences, in public health education, of attacking the problems in this way. A better example is smoking and the use of mass media for dealing with advertising problems concerning community mental health, alcohol to some degree, certainly prevention of heart disease and malignancy.

Sherri Cavan: I have difficulty following the discussion in this sense: the papers presented this morning, and looking through the one that is to be presented this afternoon, treat these two models in a sense as independent variables, the objective of which is to explain the dependent variable which is alcohol problems. And yet it is as though everyone here simply assumed they knew what con-

stituted those problems. For example, there are probably some subcultural norms that when you comply with the norms of that particular subculture in drinking by some other criteria might constitute an alcohol problem. I am wondering whether that issue is understood by everyone here—what problems, or whether in fact, that itself is going to be a topic of discussion, and if it is, would not that be a more appropriate topic to deal with in the beginning rather than leaving it until the end?

Dwight Heath: I think that Dr. Cavan's question brings us back to Dr. Whitehead's. I would say that probably the greatest value of the kind of querying—what Bob Straus might have been calling the qualitative approach to data as contrasted with the quantitative—perhaps the strongest value of this is to find out what are the important questions, and I think that the first part of Dr. Brenner's response was far more effective than I could have been in describing them. That when we talk about sociocultural words we are not just talking about small, isolated, homogeneous populations at all. Furthermore, if we look at the national institutions, I think probably you wouldn't have to go more than a mile from here to find a population where the reaction to, let us say, Nixon's Watergate capers or the burdens of Vietnam, was very different from what it has been around this table. So I think we really have to address ourselves to a variety of populations, recognizing that in each of these there may be very different kinds of problems, just as there are different kinds of attitudes, and trying to ascertain how these are learned, how these are managed, what meanings they have. What we've had in terms of methodology, you don't see in the alcohol literature. But some fairly sensitive stuff is being done on decision making, particularly in relation to agriculture and marketing. We have some excellent stuff on networks that doesn't just give us sociograms but deals with the quality and intensity of interaction and the nature of communication that is involved. The logical work I think is going in directions I consider very promising. It is that kind of thing that I suggest be moved into the arena of alcohol studies. If we are to look at the overweening importance of national institutions, I think that they cannot be ignored, but our experience in so-called noble experiment I think demonstrates that these national institutions may not be quite so overweening as we sometimes think they are.

Griffith Edwards: Harvey Brenner talks about the possible impact of the mass media in changing attitudes. To me, the live question is therefore why the mass media are not used. I think this takes us directly into the challenge John Seeley put before us. I myself do not have much faith in the possibilities of relating

alcohol problems to the Vietnam war. It fascinates me for a discussion over a glass of port, but I find it difficult to set out a series of conjectures capable of refutation. I am not particularly charmed by the Marxist analysis of drinking problems, which tells us that alcoholism is a symptom of capitalism in the late stage of decay. I don't, though, believe that we can get away with going on interminably with our cozy in-group permutations and combinations of the sensitive researches of 20 years ago, while leaving other matters with a polite and neutral bow. One could make a list of some of the matters totally neglected in current research. For instance, in Britain at present, the Price Commission, which is supposed to hold back galloping inflation, has had the price of beer referred to it, because the price of beer is thought too high. At the same time, a subcommittee of the Department of Health is suggesting that the price of liquor should be kept up. One would have to understand the relative positions and power positions of these two organizations of government. To take another example, in Britain a suggestion that random breath testing should be introduced was defeated. And it was defeated partly, perhaps, because of the influence of motoring organizations. Another matter—it would be very interesting to know what the influence of international liquor interests is in the encouragement of drinking in Africa. One might want to know why the price of liquor is in the cost-of-living index in Britain. So if you put up the price of liquor, you put up the cost of living, and therefore the unions can demand a threshold increase in wages that gives a further twist to the inflationary spiral. Coming nearer home—or home where I have the pleasure of being a guest—I would like to see an analysis of *Alcohol and Health, New Knowledge*, yet *Newer Knowledge*, as that excellent publication goes through successive editions, to try to identify what determined the content of that publication and what determined the exclusion of certain other matters. I would like to understand the relative significance and power and influence in this country of the liquor industry, advertising, the National Council on Alcoholism, and NIAAA, and the entrenched investments of political positions. I don't believe that those are matters forever to be bowed at and passed by.

Toward a Social-Psychological Perspective on the Prevention of Alcohol Abuse

Richard Jessor and Shirley L. Jessor

As latecomers to the prevention field, we have been struck by several aspects of the recent literature. First, there seems to be a situation of premature closure in the prevention debate, with discussion largely restricted to the two major approaches Whitehead (1975) has labeled the "sociocultural model" and the "distribution-of-consumption model," the former emphasizing norms and the latter, per capita consumption. Second, discussion of prevention seems to take place with little if any reference to theory, that is, to a theory of alcohol use and abuse.¹ Third, most orientations to prevention appear to be alcohol-specific, focusing on those factors that are proximal to and directly connected with drinking behavior and giving less attention to factors that are distal and indirect.² Fourth, in almost none of the approaches is there a conceptualization of persons or of variables to represent individual differences; what seems to be assumed is that one individual can be substituted for another and can be dealt with in the aggregate without significant loss of information. And finally, the near-absence of a body of prevention research is noteworthy; it suggests that prevention policy for some time to come will have to rest on insight and judgment rather than on relevant empirical data.

These impressions raise most of the issues we want to address in this paper. In dealing with these issues our aim is to provoke or facilitate discussion rather than to offer solutions. The latter are just not easily come by; despite nearly two decades of research on alcohol use, we find it a sobering exercise to try to derive a policy for prevention from a set of explanatory findings. The large gap that exists between research that is oriented toward analytic understanding and research that is oriented toward intervention has been noted by others. In a recent volume of papers on social policy, Reiss reflects that "Sociologists may take comfort from

trying to find out why things are as they are, rather than from learning how things that are can be made different. But social policy . . . is informed more by the latter than by the former" (1975, p. 220).

Norms and Consumption Levels: The Major Models

It is useful to begin with a brief commentary on both the sociocultural or normative approach and the distribution-of-consumption approach as introduction to our more general concerns.

The emphasis of the sociocultural model is on the [regulatory function of drinking norms in restraining alcohol abuse.] Norms that prescribe moderate and proscribe excessive alcohol use have been shown to characterize various cultural and ethnic groups in which drinking is integrated into social life and in which problems associated with alcohol are relatively less frequent. The prevention implication is for normative change, for the creation of drinking norms that sustain the moderate use of alcohol. Obviously norms are an important factor in behavior, and efforts in this direction may well have salutary consequences. But preoccupation with drinking norms alone is a simplistic approach to such complex social behavior as alcohol use, and taken alone, normative change can carry no convincing implications for more benign patterns of drinking.

The reasons for this conclusion are several, but they all point to the necessity for considering a larger conceptual domain than that encompassed by drinking norms alone. To assume that the quality or content of drinking norms will have a direct effect on drinking behavior is to assume a nonproblematic relationship between norms and behavior, an isomorphism that is rarely observed. The relationship between norms and behavior should instead be seen as problematic, and the issue is not simply the content of norms but [*the factors that influence the hold norms have over conduct.*] The hold norms have over behavior will vary with the nature and the effective operation of social controls and sanctions for normative departures; with the degree to which there is unimpeded access to alcohol; with the consonance of drinking norms with norms in other areas of alcohol-related behavior such as sex, aggression, and drug use; with the degree to which there is pervasive consensus versus anomie in the larger structure of norms

about social behavior in general; with the degree to which there is a stake in normative adherence via access to societal opportunity; with individual differences in personal controls; and so on.

The essential point here is that a prevention focus on changing drinking norms is conceptually parochial. It is likely, also, to be empirically unsuccessful unless attention is paid to changes in a larger variety of domains—especially to those factors that determine the regulatory effectiveness or the moral authority of norms in relation to behavior.

The distribution-of-consumption model is focused on the overall level of per capita consumption of alcohol in a population and on the linkage between consumption level and certain aspects of alcohol abuse, particularly alcoholism and liver cirrhosis. The prevention implication is for lowering the population consumption level by restricting availability through a variety of strategies including a strict pricing policy, limiting on-premise drinking hours, lowering the alcohol content of beverages, etc. Again, availability is clearly an important parameter of use, and efforts to reduce availability may well have positive consequences. But again, a focus on availability alone cannot yield unequivocal implications for more benign drinking patterns.

Rather than consider the technical aspects of the assumptions about and derivations from the log normal curve, a task assigned to others, we want to raise some other matters. First, our concern is with the absence of theory, of any mediating model that would link up a lowering of overall consumption in the population with a change in the drinking behavior of the heaviest drinkers, those who are the abusers or the casualties. The exchanges on this question at the Berkeley prevention conference (Room and Sheffield 1975) are illuminating. In response to Harford's query about the intervening processes by which reducing consumption of the vast majority of drinkers can influence those at the critical end point of the curve, de Lint states: "It is one thing to have a model which describes the realities of a situation, it is another thing to explain its mechanisms. . . . Exactly what is happening we will not know, it would require complete knowledge of human social behavior which is inconceivable" (p. 172). The frustration created by the urging of a major policy initiative in the absence of a rationale that explains its workings is reflected in Cisin's reference to ". . . some process that you don't understand, and I don't understand, and which is presented to us as a black-box process, as something in the mind of God that we are not supposed to understand—somehow this curve will shift in its entirety and not just shift at the lower level" (p. 329).

Disconcerting as the absence of a mediating model is for this policy of reducing overall consumption, the approach also seems to be inconsistent with more specific theoretical assumptions. Various analyses of drinking behavior have emphasized the rewards associated with the use of alcohol—what Sulkunen (1976) refers to as its “use value,” what Trice and Beyer (1977) call its “payoffs,” or what we (Jessor et al. 1968) have termed its “functions.” Trice and Beyer emphasize that “to intervene effectively to minimize drinking problems it is essential to start from a thorough knowledge of the rewards that occur in much social drinking” (p. 71). High use value, or payoffs, or positive functions should lead to price *inelasticity*, and there are data to indicate that the heavier the involvement with alcohol the greater and more diverse are the positive functions associated with it. Thus, attempts to reduce overall consumption by limiting availability via a tax or price policy *could well be least effective with precisely those who are abusing alcohol.*

The argument here has focused on the absence of theory in general in the distribution-of-consumption model and on the lack of attention to specific theoretical variables such as the uses or functions of alcohol. Beyond the question of whether and to what degree alcohol is price elastic, concepts like functions suggest that elasticities need to be established for heavy drinkers separately from other drinkers.

There is one other aspect of the distribution-of-consumption model that bears mention here. Much of public health policy has been based upon an “other-regarding” justification—that is, individual behavior that has direct effects upon and affects the well-being of others, such as drunken driving, is a legitimate concern of governmental regulation, whereas individual risk-taking behavior, such as drinking enough to result in cirrhosis, is not. More recently, public health concerns have been oriented toward such private choices as well, calling attention to the social consequences of the aggregated individual harms of personal risk-taking, such as using up the health resources of the society (see Bonnie 1977, for this “conservationist” rationale for public health efforts to modify life styles). What is interesting to note is that *neither rationale applies to most of the people who would be affected* by a systematic discouragement policy based on restricting availability—those moderate drinkers whose drinking has neither other-regarding effects nor aggregated social consequences. Intervention in the lives of moderate drinkers when neither of these public health justifications applies, and without any theory to link that intervention to beneficial consequences for the heavy drinkers in the population, is surely a matter for further consideration.

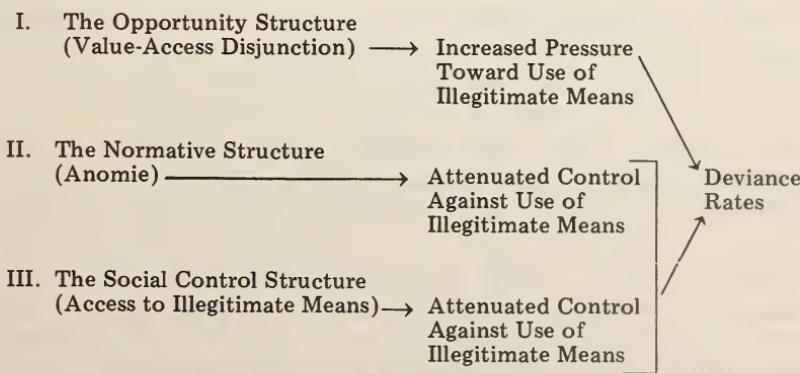
Prevention in Relation to a Theory of Alcohol Abuse

Our aim in discussing the two dominant approaches in the prevention field has been to call attention to limitations that stem from the narrowness of their compass and from the lack of theory. Each approach seems to assume a nonproblematic relation to alcohol abuse, one between drinking norms and abuse and the other between availability and abuse. Yet, as we have argued, the relations need to be seen as problematic and contingent upon some larger network of factors, a network that would include at least those factors that are emphasized by the other approach.

One such network was sketched out some years ago in the Tri-Ethnic Project, and it is apposite to the present considerations to consider a portion of it here. Figure 1 shows the sociocultural system portion of the larger framework, and several aspects of it are worth mentioning. First, the system encompasses both approaches discussed above; the emphasis on drinking norms belongs in the normative structure and the emphasis on availability belongs in the social control structure. Second, both structures have a similar function—to control against the use of illegitimate means—in this case, excessive alcohol use. Third, the effectiveness of either set of controls can be seen to depend in part on the effectiveness of the other. Thus, embedding the two approaches in a theoretical system makes clear their function and their dependence on other factors.

But something else is revealed by the theoretical system that was simply not apparent before—there is a *third* structure, the opportunity structure, the implications of which have been completely

Figure 1. The Sociocultural System and Deviance Rates



ignored by the two dominant approaches in the alcohol prevention field. Limited access to opportunity—to jobs, education, status, social acceptance, family integrity, mobility, group membership, adequate housing—was conceptualized as a source of instigation or pressure toward excessive alcohol use. The greater such instigation, the less the stake a person or group would have in the social order and the greater the attenuation of normative and social controls. The prevention implication of such a conceptualization is quite clear—it points to *increasing access to opportunity* as a way of lessening pressure to abuse alcohol.

It is possible now to see a further advantage of embedding prevention approaches in a more comprehensive theoretical network. Not only does it make clear the other factors on which the effectiveness of a given approach will depend, but it suggests entirely different approaches as well.

The emphasis we placed on changing the opportunity structure (rather than changing drinking norms or changing the population drinking level) is not represented in the current prevention literature. This fact puzzled us until we came across the remarkable “confessional” by Wilkinson (1975) about the policy choices made by the Cooperative Commission on the Study of Alcoholism. Wilkinson describes the background of the decision to emphasize drinking patterns and norms (an alcohol-specific approach) rather than to take a more radical (nonalcohol-specific) approach emphasizing aspects such as those we have alluded to in our discussion of the opportunity structure. The decision was made not on the basis of theory or of data but rather “we chose a causal model of drinking problems that seemed amenable to policy interventions. *Radical* models . . . did not seem so amenable; they involved factors that seemed too deep to change, at least by anything that we felt we could reasonably propose” (p. 374).

What is fascinating about this account is that it represents another kind of foreclosure of alternatives, one that was based not on a theory of alcohol abuse (“Ideologically, key members of the Commission staff were well disposed to recognize this deeper level of causation, but when it came to concrete proposals, nothing substantial was published about it.” p. 374), but on some estimate of its pragmatic possibilities for a prevention policy. Whether calling for full employment, equal educational opportunity, and adequate housing is really less amenable to policy initiative than calling for a change in drinking norms is probably open to argument. Clearly there is a real danger of simply accepting the status quo and tinkering with superficial aspects of the social system.

But the point we want to emphasize out of all this is the role that theory can play in suggesting a *logic* for prevention approaches—changing the structure of opportunity would seem to be part of that logic and worthy of consideration.

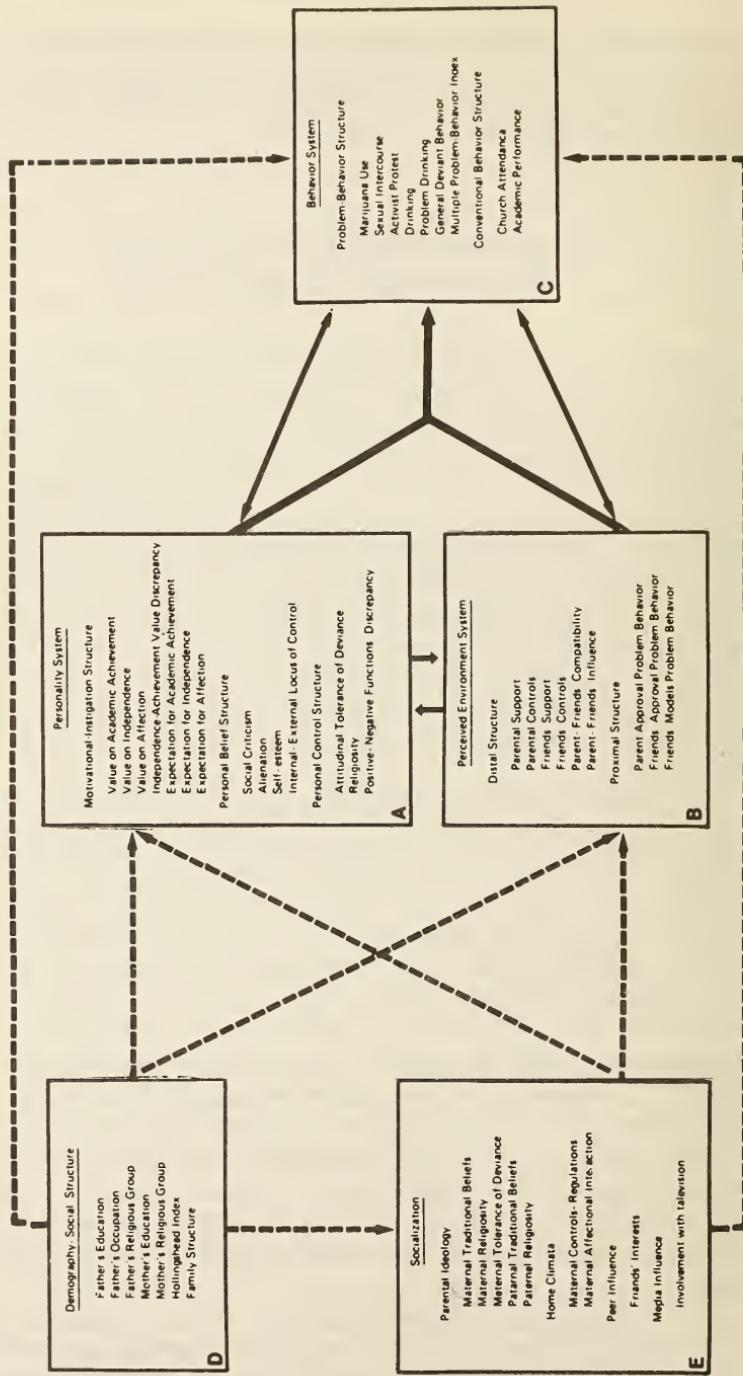
Problem Behavior Theory, Prevention, and Youth

We want to carry this discussion of the implications of theory for prevention a bit further. To do this, we turn to the theoretical framework we used in our recent, longitudinal study of problem behavior in youth. The conceptual structure of problem behavior theory is presented in figure 2. It represents a fairly complex social-psychological scheme of systems of variables and their relations to each other and to problem behavior, including problem drinking. The utility of this framework for both cross-sectional and longitudinal explanation of variation in problem behavior is developed in detail in our recent book (Jessor and Jessor 1977). Our concern at this point is not with the empirical success of the framework but with its logical implications for the prevention of alcohol abuse in youth. Here are two of these implications:

1. *Problem Drinking as Part of a Nexus of Behavior.* In problem behavior theory, problem drinking is located in a structure of problem behaviors within a larger system of behavior (figure 2, box C). The implications of this are crucial; they suggest that problem drinking is functionally linked with other problem behaviors, should covary with them, and should relate inversely to conventional behavior. Indeed, this is precisely what we found among our high school youth, and it holds for both sexes. Such findings raise a serious question about alcohol-specific prevention efforts. If alcohol use and abuse among youth are intricately interwoven with other behaviors such as drug use, sexual intercourse, and general deviance (aggression, stealing, lying), can a prevention approach afford to ignore these linkages or expect to be successful in doing so? It is worth noting that neither of the two major prevention approaches gives systematic attention to this issue.

2. *Personality and Prevention.* The second implication to be emphasized is the role of personality factors in prevention approaches. In our research it has been possible to demonstrate significant relationships between variables in the personality system

Figure 2. The Conceptual Structure of Problem-Behavior Theory
**ANTECEDENT-BACKGROUND
SOCIAL-PSYCHOLOGICAL
VARIABLES**



From Jessor and Jessor (1977). Copyrighted 1977 by Academic Press.

and the frequency of reported drunkenness or status as nonproblem or problem drinker. The strongest and most consistent relations are with variables in the personal-control structure—attitudinal tolerance of deviance, religiosity, and positive and negative functions of problem behavior; the stronger the personal controls, the less the problem behavior. What this portion of the theory and these empirical findings raise for consideration is the utility of prevention approaches that are focused on internal personal-control systems,⁷ systems that are not alcohol-specific and that would have implications for problem behavior in general. What we are referring to is socialization—development of a sense of connectedness with others,⁸ of an “other-regarding” ethic, of a feeling of social responsibility,⁹ and of what Alfred Adler used to call “social interest.” Such internal personal controls have notable advantages—they may be expected to operate across a wide variety of contexts,¹⁰ to engage a variety of behaviors, and to be effective in relation to both private risk-taking and socially consequential actions.¹¹ Given the interconnectedness among various problem behaviors discussed earlier, and the difficulty that presents for efforts that are directed at alcohol abuse alone, prevention efforts that are concerned with the development of central, non-alcohol-specific, control factors of personality seem worthy of consideration.

Such emphasis on personality is anathema to many sociologists, and the Berkeley conference seemed uncertain about whether it was proper to deal with individuals or better to focus on the population as a whole. Gusfield’s comment is not atypical: “Throughout this chapter I have cast much doubt on the utility of individualistic policies as effective prevention mechanisms” (1975, p. 104). Our own view is that every prevention approach carries with it, latent or explicit, some model of individual functioning; making the model explicit permits it to guide the prevention approach. Second, a concern with individual policies does not mean that intervention must be on an individual basis; to the contrary, social reorganization may be the most efficient approach to individual change. Third, attention to individual or personality factors makes possible a continuity between prevention concerns and clinical concerns, a continuity that could have salutary consequences for the treatment field.

Summary

Our main concern in this paper has been with the role a social-psychological theory can play in providing a perspective on

prevention. Considerations of theory were applied to the two major models for prevention—the normative model and the distribution-of-consumption-model; both models were seen to be conceptually parochial. More important, perhaps, were the implications to be derived from the framework for additional or alternative prevention approaches. Three of these implications were discussed—the role of the sociocultural opportunity structure, the role of the behavior system, and the role of personality. Our ultimate objective has been to forestall closure in the prevention field and to enlarge the arena in which alternative approaches can receive attention.

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Discussion of Paper by Jessors

Aaron V. Cicourel

I have some general comments about the methodology of the paper by the Jessors. In general I like their research, and I find myself in considerable agreement with many of the assumptions they make about the value of their proximal-distal variables. It is difficult to translate the draft of their study into a research design that is going to satisfy everyone, and I'm sure they are well aware of that. Nevertheless, I think it is important to say something about the consequences of opting for one type of research as opposed to another in order to test such a broad theory. I would like to say a few words about that in just a few minutes.

I would like to also talk somewhat about how we can interpret their theory; a theory I think is very much needed. And I can agree with the Jessors on the necessity for more theory. I have been involved with the study of social problems for some time, but have been out of the area for a while. I am amazed to hear the same kinds of remarks again.

I want to stress that many sociologists hesitate to talk about distal and proximal variables and are especially afraid of trying to combine them. Most sociologists prefer to talk about macro-variables or distal phenomena, and ignore, say, personality or proximal variables. In sociology we have seldom attempted the integration proposed by the Jessors.

In my own work I have tried to do something parallel to the Jessors' work but I don't think I have stated things in quite the same way because I come to the integration of proximal-distal variables from a different perspective—one that would probably surprise the Jessors because it is probably close to some aspects of psychology.

What I have been trying to do is talk about macrostudies and microstudies by recognizing the fact that at one level, the interactional level or interactional-personality level, you have expressions of behavior that are basically linguistic and nonverbal in form and that involve very complex information-processing strategies for individuals. On the other hand, you have macro-approaches based on summarized textual materials. These textual materials may be tables derived from distal variables, reports from organizations, or the summaries of organizational meetings such as this one here. What I have been trying to do is to show the rela-

tionship between how people talk about something and the way they summarize what they have talked about at a later date, such that the summary now becomes the basis for a macroanalysis completely divorced, incidentally, from the interactional elements that produce that macroreport. This is an important focus to keep in mind because what it says is that there is an organizational level of analysis that must move to interactional variables and vice versa. I think this is somewhat absent from the Jessors' perspective—that is, the organizational context within which their important study was done. Considering the kinds of restrictions that are placed on someone using a survey approach, I think their study is very carefully conceived and very well executed. There are not many such studies in sociology, because few sociology studies derive their survey questions from an integrated theory. Theoretical speculation usually occurs after a review of the printout of marginal data.

Being familiar with the research literature and the theory to which the Jessors referred, I was pleased that they were able to stick to that theory and follow through on their research strategy.

Let me point out what I think is a problem here—a problem that is quite common. We have a macroapproach to a problem that is mediated through variables that resist translation into what can be observed occurring day by day. We have access to many reports that describe various kinds of studies that imply that some direct observation was made of alcohol consumption or the patterns of drinking. But alcohol consumption or patterns of drinking are conceptualized at a distance because it is convenient to do so and difficult to study natural settings.

We can talk about policy on these issues forever, and we never have to worry about a solution because there is no solution in sight, necessarily, when we are at such a distance from the phenomenon itself. So what we are faced with is this peculiar situation of wanting to deal with a concrete group of people so we can address that group with a prevention program, but as soon as we move to a national picture of different groups diversely distributed in some society, we find the problem changes. We then have a huge program that gets implemented through bureaucratic endeavors that are very difficult to monitor, much less control. This is the kind of problem we are constantly dealing with, and it is one for which we have no theoretical solution. I am proposing that we follow up the Jessors' advice about using a broader theoretical base. How can we build on this in a way that makes sociologists (for example), who have a big commitment to a macroview of things, place their own interests into such a program without altering the kind of work the Jessors have done and are proposing?

It seems to me the one way that can be done is by a slight reconceptualization of some of the conditions the Jessors have talked about, trying to create a somewhat broader organizational base. We need to understand something about the way in which macroconceptions of any problem are created in the first place. That is, what kinds of processes lead us to infer a macroconception of a problem? Where do the data come from? How removed are they from possible observations, and is it possible to integrate them with primary data that are not removed, without losing the organizational perspective that comes with a macroapproach?

I think we must begin by recognizing that it is difficult to take the entire society as the object of study and simultaneously integrate these two levels. We will need more long-range goals that don't require that we solve the prevention problem tomorrow. Everyone wants immediate answers to these questions, but there are no immediate answers. Yet we fund one project after another, and one conference after another, with the hope, yet to be realized in any area of social problems, that somehow someone is going to come up with an answer. The answers don't materialize; what *does* materialize are more and more projects that claim to have a more precise representation of the problem in the larger society. I don't think we can expect much from these projects. What we have to do is tackle the problem at some organizational level that enables us to bring the macrovariables and the microvariables together in the same context. Now that context may mean only one city at a time, but I think it's better to work in one city at a time and then extend our study to a few more cities around the country, than to try to address the alcoholism problem for the entire United States. I feel we need smaller projects that address the interface between day-to-day living activities and the more macroconsequences of occupational and family-community relations.

If we are to address the prevention problem realistically, we need a theoretical basis that doesn't always require an immediate practical solution. Instead, we need to fund research on alcoholism that may not pay off for another 10 years; this work should be integrated with other basic aspects of daily life of which drinking is but one facet.

One of the problems here is that we cannot entice young researchers to get into alcoholism studies. We need to stress the theoretical significance of studying the problem, without insisting that there be an immediate payoff when they finish the data-gathering aspects of the project 2 years later. We cannot entice good minds into this kind of research if we don't give them an opportunity to develop a broader theoretical perspective, includ-

ing theoretical perspectives that don't necessarily relate directly to alcoholism. Now that is one of the messages I read into the work of the Jessors. You can't just look at alcoholism. In order to examine the matrix within which people drink, we have to look at a range of activities engaged in by people who happen to be alcoholics or who drink a lot. We are really talking about how any kind of behavior, problem or not, is related to how much money you have, where you live, whom you associate with, and about how particular kinds of problems emerge in the routine of daily life. I stress that all these problems can be seen in persons who seem to be living normal lives. Now that's a very important factor. In funding of research on alcoholism, we should not require projects that explicitly examine alcoholism or that insist on a prevention program. We need to explore ways of linking a problem related to alcoholism and prevention to more general theory and a more general methodology.

The more general methodology for me involves taking seriously how people express their feelings about alcoholism, how often they get together with people just to socialize, where drinking is a by-product. To what extent do people talk about these issues? It is not easy to study these kinds of activities, but it seems to me we can study them in an organizational context of work and leisure. We can study actual groups devoted to the prevention of alcoholism and the family, work, and leisure support systems where alcoholism is not always defined as a problem but where routine drinking takes place. We need to study the way people talk on both sides of this fence and how they summarize what is going on. How do prevention groups discuss and summarize topics related to drinking and alcoholism? How do such talk and accounts compare with routine discussions about similar issues in daily life settings where drinking occurs?

The kind of study I am suggesting will not give us prevention tomorrow. What it is going to do is tell us how members of society normally deal with these problems in prevention and everyday settings. We don't know very much about that. We need data on how treatment personnel involved with alcoholics talk to their clients as opposed to how they write up what they say to the clients. This type of data is important because it shows us that the language games being played here are not the same, that they often intend to do different things. Many times treatment and research programs utilize different forms of reasoning and semantic domains in their professional and clinical activities. The hypothesis is that they keep different language systems going and that these activities occur in parallel, with very little interaction between the two domains.

In Europe, and recently in this country, there is a growing body of researchers interested in what is called "textual analysis" or "story grammars." The data base is often a summary statement, which can mean a table, an organizational report, a clinical report, a medical report, or a doctor's medical history. These summary statements are examined to discover formal structures of summarization or comprehension. They almost always consist of hierarchical levels. The summaries are examined for the kinds of structures considered necessary for explaining whatever activities are described by the summary. This notion of story or text grammar has been used to study folk tales in a book written by Vladimir Propp. Today a number of people working in artificial intelligence projects, cognitive psychology, and linguistics have applied some of the notions from Propp to the analysis of structures in stories. The analysis of stories has a remarkable similarity to the way anyone summarizes his or her experience.

I've tried to apply the work on story grammar to the way doctors interview patients and write up medical histories about the interviews. The difference between the two types of data is sometimes quite striking. The doctor's medical-history statement is usually taken by other researchers as the basis for constructing a macrostudy—because of using the reports to extract a diagnosis; but the report also reflects the transfer of information from an interactional context to one of organizational summarization. The distal use of a doctor's diagnosis for an epidemiological study does not examine what the doctor said to the patient, and whether the patient understood the doctor. I think this transfer of information is a key organizational aspect of how social structures are produced and reproduced. Because this report is a legal basis for subsequent action, the interaction that led to its assembly may be lost forever. When another doctor comes to read that report, he uses it as a basis for inference and action. The report may also be used to do a statistical study. In fact, if you look at death certificates in the same way and ask how death certificates are assembled, you can arrive at an interesting study of how someone dies under one set of circumstances (the interactional exchanges about cause of death), as opposed to how they die officially under formal summary statements which then become the hard data about morbidity and mortality.

There is a practical side to my remarks in teaching medical students how to write a decent history and how to make inferences from a history divorced from the interaction that produced it, so that the students judge what things a physician did not say but had to imply and code in abstract ways in the text itself. Strong theoretical ideas are needed for this type of research, in-

cluding ideas from sociology, anthropology, cognitive psychology, artificial intelligence, and linguistics. Ideas from all of these areas are necessary if we are to understand how members and institutions of a society talk about a problem and how they routinely handle the problem, with or without prevention programs.

There are now many drug abuse programs, alcohol abuse programs, county agencies, State agencies, Federal agencies, and some private agencies, all dedicated to talking and writing about their clients. What do we know about how these clients are talked and written about? How are they talked to and what are considered to be key factors in a person's problems? To understand the discourse between a physician and a client, social worker and a client, or a psychologist and a client requires tedious and difficult analysis. We cannot do it by citing a few key quotations from the interview. We need a model that can represent the comprehension and production of speech as discourse and in textual structure. Unless we understand the structure of discourse, we can't possibly understand our reliance on question-and-answer strategies in social research.

Today I think we can show that discourse and textual materials are more objective than the distal variables used by macrosociologists. I think we can show that macrostatements are in fact subject to the same principles inherent in the analysis of discourse and text.

The studies the Jessor report are really important, and my concern is how we can broaden them enough to include a validity element. That validity element is: How do these junior high, high school, and college students express themselves about the ideas the Jessors examined in their survey? I'm not trying to dispute the validity of the questionnaire. What I'm saying is that the questionnaire materials tend to be more distal than proximal (more so than one might infer from the Jessors' report of their findings), because the methodology they used creates a more distal data base.

But the methodology the Jessors used is an important one to build on. It should be used in conjunction with studies that emphasize how the same youths talk about these problems to each other and to other persons around them.

A development approach is central to learning how the students talk at different ages and again when they are adults. We need to know how people express themselves about these problems at different times in their life. We know nothing about it now, and we can learn much more by building on what the Jessors have done.

We need controlled field research. This means more than con-

ducting in-depth interviews and using observational, theoretical, and methodological notes for participant observation. These provide a nice beginning, but we need an organizational context: Where do students meet, under what conditions, and with what regularity? Under what conditions do they summarize for each other their own drinking, drug, and sexual behavior? Students meet regularly in organizational context. According to the Jessors, all the age groups studied reported that 90 percent of the enjoyment of their activities came from telling people about them, even if they didn't enjoy it originally. If they can't tell someone about it, it is as if it didn't happen.

In other words, the students do not talk about drinking as if they love to go off with a bottle and be left alone and never tell anybody. Nor do they enjoy smoking marijuana, having sex, having an argument with someone, or hitting or beating someone, without telling about it. Again, how you tell is 90 percent of the game. We have neglected this part of it, and we've also neglected how we created bureaucracies for official tellings via sociologists, social workers, psychologists, physicians, and so on. We don't know much about the structure of the official tellings. We tend to treat them as if the language used was only a passive device for reporting what really takes place. Language is not a passive device for communicating information. It structures the content and form of information at every step with every verb, noun, adjective, clause, and paragraph. We have neglected the importance of cognitive-linguistic structures in organizational settings where the original problems developed; we have ignored organizational settings in which the problems were identified and where solutions were proposed and implemented.

General Discussion

Sherri Cavan: After hearing the Jessor paper, I guess I'd like to raise the question I raised this morning, and that is how we are to understand what is the problem about the nature of the dependent variables; specifically, what it is we're trying to prevent. There was considerable concern with whether the independent variable was going to be specified in terms of sociocultural factors or whether it was going to be specified in terms of controlling the availability of alcohol. The dependent variables were vaguely specified as people who drink in an abnormal manner; problem drinkers; and those who drink too much or use alcohol to excess. This all seems to assume that there is a proper amount to drink. It struck me again that what is problematic about this is that it is problematic from the standpoint of State authority. By and large it's this kind of abstract notion that suggests that young people ought not to have sex or young people ought not to engage in activist protest, even though one might argue that they are acting under their First Amendment rights. There are a variety of things young people ought not do, but we've really never addressed them. What is the source of those norms that prescribe what young people ought to do such that what they actually do can be defined as deviant? That strikes me as really germane and unless that issue is solved, I really can't get seriously behind the discussion of the independent variables and what it is that's varying.

Harvey Brenner: The issue as I understood it from Dr. Cicourel had to do with integration of macrolevels and microlevels of analysis. Now I've a problem with the theoretical structure, not with policy implications. As far as I'm concerned, the policy implications are screamingly obvious. They're not a theoretical exercise way off somewhere in the next 10 years; they are before us and have been before us for quite some time, and the theory in this area has had a very long history of development. I've a problem with theory because it appears, as contrasted with the theoretical structure we heard this morning, we're going to have something quite different. This morning we talked about normative structures as applied to some aspects of the population around which there is some variance. And we talked about the things that impinge upon changes in these normative structures, but we did not talk about things that tend to make for extraordinary deviance upon these norms—the central focus of this afternoon's papers. It is a very different theoretical orientation.

In the morning we wanted to explain what creates order in the

world, and this afternoon we want to explain what creates disorder. This, too, has a long theoretical history which we needn't encounter at this point, but the implications are really quite different and the policy implications are extraordinarily different. And I'm not at all sure that this is the only kind of distinction that should be made here.

This morning we talked about what pervades the normative structure and how it changes. And this afternoon there is an emphasis on younger people, on youth, exhibiting what appears to be deviant behaviors including alcohol consumption, and what it is on a microscopic level and on an intermediate level that seems to be associated with those behaviors, one of which was the opportunity structure. So we have two kinds of influence on drinking patterns, one we might call normal drinking patterns and one we might call abnormal drinking patterns, each of which has somewhat different sources, different causes.

There is a third possibility and that is that some deviant patterns are in fact also normative patterns. By this, of course, we mean abnormal or subcultural behaviors particularly prevalent among youth and highly subject to change over time, or particular ethnic subcultures in society or occupational subcultures in society which accept different behaviors both in their everyday life and in their drinking patterns, so that we can have simultaneous substantial deviance with respect to society as a whole and considerable amounts of normality—quite normative behavior within subgroups. So we've been talking about three types of behavior when we've talked about alcohol consumption and its implications. Each of these conceptions and the theoretical framework in which each occurs would seem to offer us quite different policy implications. I'm not sure of the third, but from my own understanding, one of this morning's implications is that it would be very nice if, for the sake of some benefit for somebody in society, some group in society would identify high risk for injury via alcohol consumption so that normative patterns might somehow be offered.

It seems to me that one of the chief implications is that opportunity structures in this society want somehow to be manipulated and there are many ways of doing this. We would do it ordinarily through our progressive tax structure. However, to say that we have some integration of theory that leads us from something distal, to something proximal and near—I wonder if that's quite correct. I wonder whether the terms used by Dr. Cicourel are not closer. I wonder if what we're talking about may simply be levels of analysis. Levels of analysis are not causal schemes. They dis-

tinguish all the cases of mental disorder, say psychosis, from the biochemistry or the structural changes in the nervous system, from a content of certain behavior from the same person. That does not mean that the higher level is causing what is going on in the lower level, or something intermediate is causing something above or below; they are simultaneous phenomenon. I think that the chart (Jessors' paper, figure 2) involves much of that kind of thing. I think that the antecedent background variables and social-psychological variables are in some instances merely identical. That is to say they are the same phenomenon looked at from different points of view. One is looked at somewhat from a small group point of view; the other is looked at from a more highly aggregated point of view. Indeed, the social-behavior variables, which I think are treated here as dependents, are again perhaps simultaneous with the ones in the middle, but seen from the standpoint of one act or behavior. Now in other cases I think there are some causal systems in operation, but it is not clear to me where the causal systems are operating as distinguished from where, in the overall schema, we just have different levels of analysis.

Griffith Edwards: I would like to say something regarding Dick and Shirley Jessor's discussion of structural opportunity. I think what they say is important here because we're having put before us a mixed menu—partly we are in methodologies, and partly we're looking at theories and much else besides. But it is important that they bring this additional theoretical perspective. It's important, and because of that it deserves to be questioned—a courtesy to friends. I would question the validity of the notion of opportunity as they put it forward. They say in their excellent paper that limited access to opportunity—to jobs, education, status, social acceptance, family integrity, mobility, group membership, adequate housing, and so on, was conceptualized as a source of instigation or pressure toward excessive alcohol use. From this comes one of the recommendations to community leaders. So three cheers, here is a flow from social science to social policy, and that doesn't happen every day.

Now, the reason I would seek very tentatively to question the theoretical construct is related to some of Sherri's questioning when she's saying "OK, what's the problem?" One wonders whether the opportunity structure is to an extent not responsible for the genesis of problems but is responsible for the shaping of problems.

For instance, one has to put against the Jessors' findings the reports from some countries that high cirrhosis death rates tend to

be found among the privileged classes or among company directors—in my own country, we have the expression “drunk as a lord.” I would like to know what the folk sayings are in this country. I suspect that as you alter the opportunity structure, you may give people different opportunities for drinking perhaps more, and for behaving in a different way when drunk. And because they behave in a different way, they may not be declared to be problematical. For example, I suspect that when you get rid of some of the manifest social evils, people are more likely to have a house in which to get drunk and are more able to afford a cab home, so that they will not get drunk on the street. So alter the opportunity structure and you get rid of public drunkenness. You don’t get rid of drunkenness, people will still fall down their own cellar stairs. The notion of permissible drunken deportment may alter so that people no longer assault others with their fists when drinking, but they may assault people with words that are equally damaging and destructive to a marital situation. It may be far more damaging to children to see constant, clever rowing between educated parents than the occasional exchange of culturally accepted blows. I want to see social injustices eliminated because they ought to be seen as morally unacceptable. I do not believe that by eliminating them I would necessarily eliminate drinking problems.

Richard Jessor: Going back to Professor Cicourel, I think that the issue he raises is very important. It’s the issue of the actor’s phenomenology of what’s going on, and it is very important to put it in the forefront of exploratory research, no matter how it’s done.

A certain amount of ethnographic work ought to, and often does, precede the development of surveys. So the format of a survey is usually informed both by theory and by some attempt to lay hold of important phenomenal dimensions of the area of behavior at issue. Some effort was made in both our studies to do this. The other point is that we use surveys in what is kind of an intermediate way. When our youth took our questionnaire, the first thing they did was to write a full page, if they felt like it, of open-ended response to what life has been like for them since the last time they took this questionnaire. We made a systematic effort to provide such opportunities throughout the survey, so in part we had the luxury of having a long enough time with our “client,” the respondent, to be able to gain access to some of the language formulations that are natural to those populations and to try to get some of that kind of information. However, insofar as that information is important, what Dr. Cicourel was saying is that it ought to be mapped into the analyses as well.

In the Tri-Ethnic Project, the theoretical formulation of the sociocultural system was explicitly derived from Mertonian notions about limited access to opportunity in the sociocultural system. Now it is important to recognize that when one does that one is engaging other kinds of intellectual responsibilities and not just opportunity. The thing I want to emphasize in relation to Professor Brenner's remarks is that the policy implications of the tri-ethnic work reside in all three theoretical structures. They reside in the structure of norms, in the structure of social controls, and in the structure of opportunity. For example, there is no reason why intervention about social control can't be organized in a community just as well as intervention about opportunity. In our own theorizing, we did not give causal precedence or priority to the opportunity structure; we gave it to all three structures equivalently, and that opened up at least three avenues of entry into a social system for effective change. What is important to emphasize is that changes in any one structure affect the others. Going back to the Mertonian, and actually earlier to the Durkheimian notion, the issue of opportunity is not divorced from the effect restricted opportunity might have on the tenability of norms and the hold they have over behavior. So I want to enlarge the question about the policy implications that exist in our scheme beyond the opportunity structure alone.

Now the question about drinking norms themselves, and the differentiation among the norms, is important. There is a tradition that stems from anthropology, and perhaps sociology as well, in which there is heavy emphasis on the integrity of subgroup normative structures as if they existed in isolation from the larger normative system within which those subgroups function. We would want to emphasize that in looking at any set of norms—subgroup norms, subcultural patterns, and so on—one needs to consider several things. One is that those norms operate in a larger normative framework, and they ought not to be seen as entirely independent of the larger framework. In fact, to some extent what sustains subgroup norms may well be their opposition to the norms of the larger context. We see that in dealing with youth; the sustenance for youth norms is often the fact that they are polar opposites from, and serve to emphasize the repudiation of, the norms of the larger society. That means, of course, that one has to take those subgroup norms into account. But it also means that one has to recognize that their character is not independent of the larger normative context. Second, norms are undergoing change. We often have sentimental, romantic notions about norms as if they were handed down among the Italians, the Jews, and the

Indians, and characterize them forever. On the contrary, norms are changing constantly. So the question of the insularity of norms and the question of the stability of norms are both at issue.

Now I wanted to bring this down to Indian drinking, which was involved in our Tri-Ethnic Project. Indians didn't go around saying that drinking was great, that getting drunk was great, or that there was nothing wrong with it. As a matter of fact, we were prevailed upon immediately to help the tribal council deal with the consequences of Indian drunkenness. What I want to note is that while it is important to recognize ethnic, youth, age, or sex variation in norms, it would be a mistake to oversolidify those, to give them a kind of permanence, or to give them an insulation from the larger normative system.

The final thing I want to comment about is what we mean by "problem behavior." Certainly we do *not* mean only cirrhosis of the liver. This again has been a characteristic of a lot of the discussion in alcohol, whether prevention or explanation, as if cirrhosis of the liver, being hard data, were really the best way to symbolize or characterize alcohol problems. As a matter of fact, I think it is arguable that the bulk of the problem lies elsewhere in social disruption, in the lack of personal fulfillment, in the conflict, in the suffering and so on that characterizes social life in general. Therefore, the issue of what we mean by drinking as a problem, or drunkenness, lies not so much in that kind of drinking that eventuates in cirrhosis, but in that kind of drinking that creates problems or difficulties for individuals in achieving their own gratifications and managing satisfactory social and interpersonal relationships.

Access to opportunity may well be one of the things that is related to the general problem of alcohol abuse in this larger sense. Certainly we saw that in the tri-ethnic community, Indians had nothing to do. In order to get a job, one had to go to Los Angeles or San Diego. The conceptualization was that those factors were probably systematically related to drinking, to getting drunk, to not having to get up to face a job the next day because there were no jobs, to not having anything to do. The issue of what access-opportunity might mean in relation to this seemed palpable and quite clear. I think there is sufficient basis in our explanatory thinking about problem drinking to make changes in the opportunity structure a high-priority agenda item for social policy and social change.

Paul Whitehead: It seems to me that one of the most important sets of questions, from a prevention point of view, is whether one can identify certain programatically relevant or manageable

variables that are associated with these rates of damage and whether one can bring about some changes so as to have reduced rates of damage or rates of damage that don't continue to increase as they have. Insofar as the narrow conception of the sociocultural model came about, that was just an attempt to take what appeared to be the dominant thinking in the literature concerning various social, cultural backgrounds associated with supposedly low rates or high rates of alcoholism, variously defined, and to see whether in fact those variables seemed to be associated with what were considered damaging conditions in different societies. Basically, that was then the Ullman hypothesis, as amended by Blacker, which suggests that if drinking practices are well integrated into a culture or everybody seems to know whether rules are rather well defined in terms of whether and how one should drink and the various contexts of drinking and so on, you should have a low rate of alcoholism. What we found was that in fact the explanatory power of that particular hypothesis, as we were able to operationalize it, was not very great. And so the question comes up, is there some constellation of variables that tends to have a greater degree, a fairly sizable degree, of explanatory power?

Robin Room: I think that Sherri Cavan raised a very important question, and it is really fundamental for us to spend some more time on it. What strikes one in examining the literature on the two major traditions represented by the papers at this conference—the sociocultural model and the distribution-of-consumption model—is that they have been directed at different kinds of problems. Although they both referred to alcoholism as their target, one of them operationalized alcoholism in terms of cirrhosis mortality, and the other much more in terms of the social disruptions associated with drinking. Thus both models may work at the same time, because they really are trying to prevent rather different things.

There are, it seems to me, a whole variety of different kinds of problems that someone in our society wants to prevent. It isn't at all clear to me that alcohol problems are always defined in terms of what is problematic for the state, because I don't think the State can be seen speaking as a single voice in this area, and one can also argue that in fact alcohol often serves very effectively as the opiate of the masses. Certainly you can wonder about State policies in Eastern Europe—the mote is always a little easier to see in someone else's eye—whether the ready accessibility of alcohol in Eastern Europe may not have its functions for the State. So that it's not at all clear to me that the State's interest lies on one side or the other of the availability question.

Irrespective of whether we are looking at it from the point of

view of State interest, the prevention of alcohol problems has to do with valued human behavior, and this is inherently a political matter. Sometimes we recognize this; for instance, we hold a political argument over the issue of whether the drinking age should be lowered to 18. Some problematic drinking behaviors are not seen as political but rather as private problems simply because they are outside the realm of what we've agreed to treat as political. We don't treat as a political statement a 6-year-old getting drunk, whereas we may treat as a political statement an 18-year-old getting drunk. What we define as a political issue is in itself a marker of where there is a sufficient organized fraction in the society or a sufficient disagreement in the society for the behavior to be considered a public and political matter.

One of our problems in thinking about preventing alcohol problems is that we always tend to lock them together and to think of them in terms of a single entity. Social scientists, at least, tend to be increasingly uncomfortable these days about using a one-lump notion of alcoholism. So people tend to retreat to other one-lump dependent variables that we are going to try to diminish in some way or another. If it's not "problem drinking," then it's called "damage;" if it's not "damage," then it's "drinking too much." But really, if we talk about the major issues that are subjects of popular and political concern, probably at the top of the hit parade would come highway safety, because people care a great deal in this society about drunk driving. A good deal of the driving-safety literature is organized around the notion that drunk drivers are the heart of the problem. Another issue that is of great concern to the society is youthful drinking, and that's far more obviously a political issue. The kids and their parents are not likely to view the process of growing up in exactly the same way. Within a span of a few years, there's a good deal of pulling and howling about what age is appropriate to do this or that or the other behavior from the point of view of the kid and from the point of view of the parent, and there's a good deal of patterned evasion of norms and looking the other way that goes on around that embattled area.

Clearly when you look at the literature on preventing alcohol problems among youth, it is very rare to find the recognition in that literature that we might want to be trying to prevent something other than drinking per se. The fundamental emphasis in the youth literature is still that it is drinking per se that is to be prevented or at least staved off. If you start from the analysis Dick Jessor was presenting and start thinking about preventive mechanisms, then you're implicitly thinking about trying to

prevent or at least stave off drinking per se because that is what is measured in the charts as presented to us in the slides.

It seems to me that the major areas people are concerned about with respect to alcohol are what you might call casualties—that is, things that are reputedly accidents of one sort or another—and crimes, or accidents that were intended. The strongest concern here is with serious events that involve a loss of life, loss of property, or a loss of bodily functioning. There are also problems of social role—failure to perform a social role with respect to work, family, or what is expected by society; problems of demeanor—a merchant will get very upset about someone being drunk on his doorstep, for example, because people don't like to have to step over a drunk in order to get into the shop; problems of health, such as cirrhosis; and other long-term physiological consequences. And then there are problems having to do with one's own feelings about oneself, including the organizing principle of the traditional disease-concept of alcoholism—that is, the feeling of loss of control over behavior, the feeling that one is not able to control one's own behavior in one's own life.

Now the preventive strategies you might think of engaging in for these different kinds of problems are quite different. One way or another you want to get people not to drink heavily if you are trying to prevent cirrhosis, with "heavily" defined primarily in terms of the amount of alcohol consumed over a long period of time. If you want to prevent drunk driving, on the other hand, you want to prevent the drinking of alcohol on specific occasions, right before driving. If you want to prevent problems of public demeanor, you may want to provide a place where people can get drunk in private. If you want to prevent casualty problems and social problems in the short term, you may well want to be looking at insulations around the behavior or social reaction to the behavior as well as the behavior per se. Whereas, when you are trying to prevent cirrhosis there's not much point in devising any strategy that isn't going to have an effect on the behavior. Clearly, strategies that will help with respect to one problem may hinder with respect to another problem. It is quite possible to do something that will get the society to be more relaxed about drinking so that behaviors aren't defined as problems anymore and there's less social pressure around drinking more—and end up with more cirrhosis. So you have your choice of problems, to some extent, according to the preventive strategies you adopt. It makes life very complicated, but it seems to me that the fundamental point is that alcohol problems prevention is explicitly political, and that you are not talking about a single problem when you talk about trying to prevent overuse of alcohol.

Wolfgang Schmidt: I would like to draw your attention to the great diversity of problems related to heavy alcohol use and to the probability that a preventive measure tailored to reduce one type of problem may be entirely ineffective with respect to another. Of particular interest in this regard is a differentiation between acute and chronic problems. Some investigators have proposed, for example, that reduced access to alcohol may have a beneficial effect on the incidence of chronic health problems but may not be effective with respect to the occurrence of drunkenness.

The adoption of a specific preventive measure will therefore have to be based on an assessment of the relative magnitude of chronic as against acute problems and, more importantly, on an assessment of the trends in these problems. It is my impression that in North America, at least, there does not seem to be an increase in acute problems. On the other hand, the problems resulting from chronic heavy use seem to be rising rapidly. For example, cirrhosis is the most rapidly increasing cause of death in Ontario as well as in most other parts of North America. It is the third leading cause of death for males between the ages of 25 and 64 in Ontario and New York State, and in fact in many other jurisdictions in the Western world.

It is important to remember that changes in the number of deaths from cirrhosis are very satisfactory indicators of changes in the number of chronic, heavy alcohol users in a population. Thus if these data on cirrhosis mortality represent genuine long-term tendencies, we would predict that problems related to chronic use will be the major problems in the future.

Richard Jessor: I think that Robin's point about the political nature of decisions about prevention or decisions about what ought to be prevented is very important and is something to which we must remain sensitive. Those kinds of decisions lead to, or implicate, value premises quite clearly. That is, they don't stem from knowledge of the causal structure of the phenomenon or a theory about it. They really implicate other kinds of considerations, and I think one has to recognize that to make prevention decisions is to take a particular value position, and one ought to be sensitive to the political commitment that carries with it. I'll give you an example. It is clear that the young people who remain abstainers do not get into these other kinds of things that create problems for them or for the larger society. While that's clear, it's also clear that in terms of a number of our measures, those who remain abstainers have other characteristics about which we may have value positions as well. For example, the kids who are more precocious in starting to drink, using drugs, and having

sexual experience are kids who value being independent much more. Independence is an important value in our society. We try to rear our children to be independent. The kids who are precocious are more critical of society. They have more integrated beliefs about what's wrong; they were more opposed to the Vietnam War; they were concerned about equality of educational opportunities; they were concerned about sexual discrimination; and so on.

We tend to think that that's a good product of socialization in society, to raise youth in a way that leads them to examine the society in which they live instead of accepting it. It is the abstainers who tend to see the world as much less problematic. They tend to have a moral outlook that is more relativistic. Instead of saying something is right or wrong, as the abstainers tend to do, those who are more precocious are willing to say that it depends on the circumstances; they had a relativistic morality which we usually treat as a more mature state of morality, rather than less. If you look at it that way then, those who are abstaining are not manifesting many other values that we try to implement in our child rearing and in our long-range social goals about the kind of society we want to have. Well, it's a value decision. Do we opt for trying to keep kids from drinking if it means that it's going to insulate them from having a critical outlook and from being relativistic about morality?

The other point I wanted to make stems from our interest in thinking of problem drinking, or excessive drinking, as one part of a larger system of behavior. In a national sample study, we see that those young people who are problem drinkers are also the ones who are heavily involved with drugs. So we find that heavy drinking covaries with heavier involvement with marijuana and more frequent use of other drugs—amphetamines and perhaps hard drugs. The question one has to ask is this: What is the significance then of focusing on alcohol reduction? It raises a logical possibility that if one would reduce alcohol use, one might increase drug use.

What I'm saying about prevention is that thinking about any of the things we are trying to prevent in relation to the other behaviors associated with them ought to give us pause about a specific targeting on one behavior, because we're not really sure what the ramifications of that might be for the other behaviors involved.

Shirley Jessor: What might increase the possibilities for affecting a lot of areas in which there may be problems are changes in the generalized attitudes people hold about drinking. This is not

"drinking or not drinking," but how to drink; I'm bringing back the words "responsible drinking." Some generality could ensue from that attitude so that other areas of behavior beyond problem drinking may be affected, such as drug use and adolescent pregnancy. If one is concerned with the consequences, the unintended consequences in fact, then this generalized attitude can have a host of different effects among behaviors that may be related, or may covary.

Another point I'd like to make is about discontinuation of drinking. Some people who are labeled as problem drinkers, youthful or otherwise, do voluntarily discontinue. They stop being problem drinkers, or they become good moderate drinkers, or they become abstainers. And perhaps some implications of prevention strategy could be culled from a better knowledge of what goes into voluntary discontinuation.

Robin Room: Yes, I wanted to talk briefly about the whole process of changing norms and to look at the little social experiment that is going on around us with respect to two other liquids—water and gasoline—the use of which the State is currently trying to limit.

One of the simplest things you can say about preventive strategies is that changing attitudes and changing norms with respect to behavior is probably, relatively speaking, a long-term strategy and one that should normally not be thought of as something you can evaluate this year or next year. Certainly if we are going to build norms around responsible decision making about drinking, or establish a norm in the United States, that seems like a long-haul proposition. On the other hand, raising the tax rate on alcohol is, at least apparently, a short-term proposition—in some countries shorter than in others. In England it's a matter of the Chancellor of the Exchequer announcing it. But it isn't always that easy, and I think we need to explore the social context of tax-setting in greater detail.

Educational programs to change norms often seem to be unpromising. Certainly alcohol education in the traditional American high school doesn't seem to be a strategy that has changed the society's drinking patterns very much over the last 80 years, since it's been a required part of the high school curriculum. On the other hand, norms can change very quickly, without official intervention. Our norms with respect to marijuana changed astonishingly quickly without anyone in particular—or without any government agency, at least—having made a normative issue of it in the direction that the norms changed. I can remember in the early 1960's that Berkeley was considered as a seedbed

of what was seen in the rest of the United States as far-out political thinking. People who were then proposing the decriminalization of marijuana were outside the political spectrum—this was a totally inconceivable proposition to put forth in a serious discussion. There have been enormous changes in our normative perspectives in a short period of time. Current examples of water and gasoline policies suggest ways of altering norms that are not organized around a model of influencing individual decisions in a rationalistic framework. Certainly when I go into a restaurant in San Diego and they don't serve me a glass of water, that isn't in fact saving a great deal of water, but it is making a symbolic statement, and it is raising our consciousness every time we go into a restaurant that this is a serious matter for Californians, particularly northern Californians. There has been a change in people's behavior in a very short period of time. Water consumption has dropped on a voluntary basis around the San Francisco Bay area by 25 percent.

When we consider gasoline and Carter's energy program, it is true that there's a great deal of individual variation with respect to gas consumption; some people are heavy on the gas pedal and some are light on the gas pedal. And we can conceive of mounting a campaign to teach people to use less gas as they drive, or a campaign to teach people to drive less. These campaigns are unlikely to have short-term effects, one would guess, but they are a symbolic statement of one sort or another. It's clear that when Carter looked at them, he decided that a structural approach made more sense than concentrating on the individual variations—that it made more sense to require General Motors to produce cars that went so many miles a gallon. Affecting the gas mileage of the car could affect the quantity of the liquid that was consumed without changing the individual behavior of the driver.

What I'm suggesting is that we need to broaden our framework of conceptualizing about ways in which a society or an interest group in the society, including the government, can go about the purposive changing of norms. It's clear that norms do change; it's not at all clear how that happens.

A Critique of the Distribution of Consumption Model of Prevention*

Douglas A. Parker and Marsha S. Harman

With the publication of Alcohol Control Policies (ACP), the question of alcohol availability has once again been placed on the agenda for scientific and political discussion. An international group of researchers argues that the rate of cirrhosis of the liver varies directly with the level of per capita alcohol consumption, which in turn varies inversely with the relative price of alcohol (Bruun et al. 1976). Raising or maintaining the price of alcohol relative to disposable personal income in order to effect a reduction in the rate of alcohol-related health problems is recommended to public health authorities and other administrative agents of the State.

That this recommendation has not gone unnoticed is indicated by the NIAAA draft forward plan proposal to stabilize per capita consumption of absolute alcohol (NIAAA 1977). The influence of the ACP recommendation is also evident in an earlier NIAAA draft position paper prepared by the staff and consultants of the Division of Prevention (Room 1976). In an extension of the ACP recommendation, the NIAAA group argues for a "broadened conception" of prevention targets and strategies involving new kinds of institutions and agencies, such as (1) city-planning and zoning boards that would control the sites of taverns and liquor stores and other conditions of sale; (2) product safety commissions that would evaluate products such as appliances in terms of their safety for the intoxicated as well as the sober user; (3) alcohol-control agencies and taxing authorities that would control the hours of sale, the type of sales places, and the sales prices; and (4) community development organizations that would limit the concentration of liquor stores in poor neighborhoods and devise alternatives to a leisure-services economy.

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The ACP and NIAAA Prevention Division recommendations are aimed at changing alcohol consumption without focused efforts directed toward altering drinking attitudes and norms. These recommendations stand in marked contrast to a decade of prevention ideology that called for educational tactics and mass persuasion designed to convert people to styles of "responsible" drinking (Plaut 1967; Wilkinson 1970; Chafetz 1974). The prevention ideology of the 1960's and early 1970's suggested that the experiences of the Jews and other groups whose ethnic heritage proscribed heavy drinking, prescribed moderate drinking, and had low rates of alcohol-related problems, could be translated into a program of prevention. The prevention spokesmen maintained that the societal normative structures could be manipulated to affect drinking attitudes and thereby reduce alcohol-related problems. Others were more skeptical, however. As Blane put it: "to expect that a particular practice, rooted in cultural-religious tradition, can be transplanted and flourish in secular, pragmatic America is naive" (1976).

Perhaps in part because of partial exhaustion of the prevention thrust of the previous decade and in part because of a growing body of research, much of it from Canada, there has been gradual acceptance of what has been termed the "distribution-of-consumption model of prevention." Many recommendations of ACP and of the NIAAA Prevention Division draft position paper can be seen as being logically derived from the basic propositions of the model.

In this paper, we examine the conceptual framework and statistical foundation of the distribution-of-consumption model of prevention. Our focus is necessarily selective and thus we do not attempt an exhaustive review of all materials and studies that bear on the model. After a restatement of basic propositions of the model, we argue that (1) the empirical evidence for the statistical properties is inadequate; (2) a two-parameter curve is necessary for adequate description of most modern, heterogeneous populations; (3) the conceptual range of independent and dependent variables is overly circumscribed; and (4) an alternative model that incorporates more variables and relationships is needed.

The Distribution of Consumption Model

Evidence had been assembled (Bruun et al. 1976; Schmidt and Popham 1975; Schmidt and de Lint 1972; de Lint and Schmidt 1976) from mortality and morbidity studies to support the

propositions (1) that there is a direct relationship between heavy drinking and premature death, and (2) that there is a direct relationship between heavy drinking and physical illness. Specifically, it has been documented through clinical and nonclinical samples that heavy users of alcohol have a high risk of premature death, although the mortality ratio may vary with the mode of identification of such users; that alcohol has an etiological role with respect to deaths from liver cirrhosis, accidents, and cancer of the upper digestive and respiratory tracts; and that other factors associated with heavy drinking, such as heavy smoking, poor nutrition, and emotional stress, contribute to the elevated death rates from tuberculosis, lung cancer, suicide, and cardiovascular diseases.

The criterion for heavy drinking that is significantly related to mortality and physical illness has not been firmly established, but the problem has been approached from the standpoint of the lower limit of consumption of treated alcoholics (Bruun et al. 1976). It is clearly possible to determine the average daily consumption as reported by alcoholics at admission to treatment and employ this as a criterion for heavy drinking. Schmidt, de Lint, and other proponents of the distribution-of-consumption approach to prevention utilize this procedure in their work (de Lint 1974a).

Third and fourth propositions are (3) that the distribution of alcohol consumption is lognormal in all populations and (4) that there is a constant relation between per capita or mean consumption and the prevalence of heavy alcohol use (Bruun et al. 1976). Because a normal curve extends symmetrically above and below zero, it would seriously misrepresent consumption, which has an absolute minimum of zero. A lognormal distribution uses either the natural or the decimal logarithms, all positive, of consumption, and is continuous, unimodal and positively skewed. It is displayed in tables where the abscissa is the amount of alcohol consumed, the ordinate is the percentage of consumers, and the distribution is skewed with its tail on the right indicating that a relatively small percentage of consumers are responsible for a substantial proportion of the total alcohol consumption (de Lint and Schmidt 1971a). As contrasted with the symmetrical normal distribution, this skewed, unimodal curve better fits the expectations about consumption: that some persons are abstainers, that many persons are moderate drinkers, that few persons are heavy drinkers with high risk of alcohol-related disease, and that the higher the mean consumption of a population, the larger the proportion of heavy consumers.

Existing data appear to support proposition 3 as they do the first and second propositions of the model. Recent examination (Skog 1977) of the samples on which the lognormal assumption was based seems to indicate that the curve is unimodal and positively skewed but that it may not be lognormal in nature. In addition, as we explain in the third section of the paper, further analysis may reveal that some population curves mask or obscure the curves for certain subpopulations, with the consequence that certain prevention strategies will lack specificity of aim.

The fourth proposition, which will be treated at length in the next section, states that knowledge of the mean consumption—one parameter of the distribution of consumption—is sufficient to predict the other parameter, the dispersion as measured by the standard deviation. There is “a certain invariance” in the distribution pattern, ACP alleges, in that differences in the dispersions of populations with similar levels of consumption are “quite small.” Thus, it is claimed that knowledge of the magnitude of increases and decreases in mean consumption is sufficient to predict the proportionate increases or decreases in the prevalence of heavy consumption.

If the mean predicts the dispersion, then the analytic task is greatly simplified. We need only ascertain the gross consumption as indicated by official statistics and divide by an estimated number of drinkers in order to compute the mean or per capita consumption and then proceed to calculate the approximate proportion of drinkers who are, according to previously given criteria, heavy consumers and thus at high risk for developing certain physical health problems. Unfortunately for all of us, as we shall explain below and as others have noted (Miller and Agnew 1974; Smith 1976), the task is not that simple. As Skog (1977) puts it, neither the dispersion nor the proportion of heavy consumers can be uniquely determined by per capita consumption.

Two further propositions are (5) that the incidence of physical health problems can be reduced by lowering per capita consumption and (6) that per capita consumption can be lowered by raising the price of alcoholic beverages relative to disposable income. Both of these propositions will be treated in the third section of this paper, but we will observe for now that it has not been documented that lowering total or per capita consumption would have an effect on the prevalence of heavy consumption, although it might have such an effect on incidence. As Miller and Agnew point out (1974), it is an open question whether the effects of a policy change will be distributed across consumers. If prices increase, light and moderate drinkers might reduce their con-

sumption, while consumption by heavy drinkers might remain unchanged.

Evidence for proposition 5 is given by demonstrating a relationship between per capita consumption and cirrhosis that is similar in strength to the one demonstrated between heavy consumption and cirrhosis. A strong zero-order or total correlation has been exhibited (de Lint and Schmidt 1971b; Popham et al. 1975), but additional studies are needed that will permit age and other factors to be partialled out and will allow specification of causal mechanisms. Brenner (1975) has shown in a trend analysis of U.S. data that for males of both racial groups and for white females there are increases in cirrhosis mortality approximately 1 year after national increases in alcohol consumption, while for non-white females the lag is closer to 2 years. This lag could be interpreted to mean that at least one variable intervenes between the long-term relation of per capita consumption and cirrhosis.

Proposition 6, the availability proposition, is often discussed in a normative vein. De Lint, for example, declares that "the first priority in government-control measures should always be to reduce or stabilize alcohol availability" (1974b). Evidence that price control through the taxing mechanism of the State has an effect on per capita consumption and thereby presumably on the proportion of heavy drinkers tends to cover periods during which price relative to disposable income has decreased and consumption increased (de Lint and Schmidt 1971b). Put another way, much of the evidence is of trends in one direction. There are other problems of a more serious nature, which will be identified below.

Statistical Difficulties in the Model

The work of Ledermann has been used as the foundation for the distribution-of-consumption model. It is therefore important to examine his original work (Ledermann 1956) carefully inasmuch as later distribution theorists (de Lint and Schmidt 1968) based their analyses on Ledermann's seminal formulation.

In 1956, *Alcool, Alcoolisme, and Alcoolisation* contained the article "Repartition des Individus Selon Leur Consommation," which put forward these notions: (1) the decimal logarithm of mean alcohol consumption is distributed normally; (2) minimum possible consumption, d , is zero centiliters of absolute alcohol per day or year; (3) maximum possible consumption, D , was taken to be 100 cl absolute alcohol daily or 365 liters absolute alcohol annually; (4) individual average consumption can be represented

by m , the population's average consumption; and (5) knowledge of these three parameters—minimum possible consumption, maximum possible consumption, and average consumption of the population group in question—determines the specific lognormal curve describing that population's distribution of consumption. By fixing values of minimum and maximum consumption at $d = 0$ and $D = 365$ l, the curve effectively becomes a one-parametric curve where only m varies.

Ledermann's equation for the distribution of alcohol consumption in a population (1956), (de Lint 1974a), was the starting point for his analysis. The equation was $t_s = aZ_s + \theta$, where t_s is the student's t distribution, aZ_s is the log transformation of consumption for the distribution, and θ is the upper limit in the distribution below which are found at least 99 percent of the consumers. While aZ_s , the log transformation of consumption for the distribution is a routine matter, the determination of θ is critical for the analysis. The larger the value of θ the longer the right-hand tail of the skewed distribution and thus the greater the proportion of the population defined as heavy consumers. De Lint assigns as θ the fixed t value of 3.43, but Ledermann, in rough translation, states that "only experimental data allow us to give an indication of the approximate value of θ " (p. 265). For Ledermann, θ is not a given but a variable whose value, at best, is approximated through the use of the weighted means of the thetas he derives from the data of six samples collected in France and the United States, which he reviews.

The first sample Ledermann examined consisted of 136 men, aged 45-65 years and hospitalized for general medical problems, and 138 men aged 45-65, hospitalized for cancer of the upper digestive system or lungs (Ledermann 1956). The method of determining daily intake is not mentioned. These men, however, are not necessarily typical of drinkers without major disease processes. Therefore, their contribution to the value of θ may not be one from which large-scale generalization can be safely made. Inspecting the data, it can be determined that the fit between the data plot and the line predicted is best where per capita consumption was high, at 30 liters and higher annual consumption.

The second sample was smaller: 95 individuals who were Italians in America or Americans of Italian heritage. As Smith (1976) breaks it down: (1) 43 Italians residing in the United States, (2) 26 Italian-American men, and (3) 26 Italian-American females. No methodological details are provided. The Italian men residing in America show a different θ from the Italo-American men, but are similar to the women. The Italians use mostly wine, he reports,

while the Italo-Americans have moved to other beverages. Only occasionally do the values predicted by Ledermann's equation fit the reported values as closely as in the first sample.

The third sample consisted of the DOXA study (Luzzatto-Fegiz 1952) in Italy. Only summary data were available, from which Ledermann made estimates to test his equation. It appears that mean consumption data were not available. As stated by Ledermann, we are dealing with wine consumed at meals, probably underestimating total consumption if data are accurate and if they are used as is for full-consumption data. The data are used to find two t -values in the distribution, but with only two data points no variation from the predicted line is possible and thus the finding is of no utility.

The data from the fourth sample provide blood-alcohol levels of 1,750 drivers stopped at random for blood-alcohol measurement during the years 1935-1937. No specifics of sample selection or of the population from which the sample was drawn are provided by Ledermann. The data are partitioned to four levels, 0, 0.04, 0.10 and 0.15 g/100 ml blood alcohol, and show reasonable fit to a lognormal distribution line. However, this data set provides one-time blood alcohol and not average consumption, which invalidates use of this sample, at least until it is demonstrated that there is a one-to-one correspondence between blood-alcohol levels and mean consumption.

Data from the consumption survey of Dr. Tara (cited in Brunaud 1955) form the fifth sample and were obtained during consultation in a "social security" office. The data relate to individuals who were seen for "professional illness." Again, blood alcohol was measured and not average consumption over any time period. The data show variation by men's age group and by sex. Fit to the lognormal distribution shows variation and θ is set from 2.4 to 2.6 using this data and setting maximum consumption at 0.4 g/100 ml blood alcohol.

A sixth sample of 247 men, aged 35-64 years, was studied by the French Foundation for the Study of Human Problems in 1943. Average daily wine consumption was recorded, and the 93 men who were still wine drinkers (154 of the 247 had given up wine for other beverages at that time) consumed an average of 161 liters of wine annually. The wine consumption of the sub-sample of 93 wine drinkers fits the lognormal line well, for the most part, corroborating the result from the first sample. Theta was set at 4.2 from these data.

The unweighted mean θ from the 6 samples combined was estimated to be $\theta = 3.31$ with $F_D = 99.95$ percent. The weighted

mean produced $\theta = 3.43$, $F_D = 99.97$ percent. However θ was observed to vary from a low of 2.4 to a high of 4.2. Ledermann then clearly states, "Nous adapterons *provisoirement*, pour les calculs généraux, la valeur $\theta = 3.43$ [emphasis added]" (p. 274) or as Smith (1976) translates, θ is set at 3.43 "provisionally" or "temporarily." The value of 3.43 for θ is not a given, as presented and used in de Lint's work (1974a), but varies considerably based on both the mean and dispersion of consumption for samples and populations. One must remember, we are taking values of t within the limits of 0 to θ . The range from 0 to 2.4, the low of θ , is roughly half the length of the range from 0 to 4.2, the high of θ , and the density function of t and amount of skew varies accordingly.

The original data used to test the proposed lognormal model of alcohol consumption show many deficiencies. There is little consistency in the data measured; the samples show poor or no description of selection techniques. Most of the samples consist of individuals who clearly, or by implication, have come to official attention due to alcohol consumption or other health problems.

Interestingly enough, these poorly assembled data still demonstrate that, if Ledermann's techniques for approximating θ are correct, θ may have a wide range of values. The age and sex differences found time and time again in daily and annual consumption data are indicated in the differences of consumption for younger men and women as compared with middle-aged men (see table 5.1.4, p. 272) where 68 percent of men 15-39 years and 71 percent of women 15-69 years had less than 0.03 g/100 ml blood alcohol, while 60 percent men of 40-49 years and 57 percent of men of 50-56 years showed this level of blood alcohol.

No evidence has been presented that blood alcohol levels are Ledermann-lognormal distributed nor that they are so perfectly correlated with annual consumption as to be safely substituted for annual consumption (Skog 1971, 1973). One suspects blood-alcohol levels would show more day-to-day fluctuation than annual consumption, in which the summarization helps mask variation. Also, most blood-alcohol studies are of highly selected samples such as drunk drivers or clinically identified alcoholics, and therefore are not representative of the general population. Thus, blood-alcohol studies can either be used with great caution or, better, discarded for the purpose of studying the distribution of consumption in the general population.

The cumulative impression from the foregoing is that—

1. The samples used by Ledermann leave so much to be desired, methodologically, that it is unwise to rely on his estimate of θ .

Also, the French and Italian sources of much of his data most likely seriously overestimate θ for lower consumption groups.

2. Theta is probably not fixed, but varies with the consumption level of the population. Ledermann's data give a crude indication of this in the range obtained for θ .
3. Maximum consumption, D , is set too high. The highest clinical sample intake of alcohol reported by Ledermann is 229 liters annual consumption of absolute alcohol, while D has been set at 365 liters, a clearly fatal but overly large choice. A review of the large number of clinical studies since 1956 should lead to a more realistic setting of the value for D . This would help avoid the error, recognized by Skog (1971, 1973) and Smith (1976), of overpredicting the number of heavy consumers, particularly in low-consumption countries.

The work of de Lint and Schmidt (1968) shows a similar result for combined wine and distilled spirits consumption in which the Ledermann equation underpredicts the proportion of those consuming less than 20 bottles per month and overpredicts those consuming 20 or more bottles per month. While the errors are not large in absolute terms, at the highest consumption (40 bottles and over) de Lint and Schmidt show 0.36 percent, while the Ledermann equation predicts 0.45 percent, or 25 percent more than the observed proportion of cases. Also, the errors are consistently under for the group consuming fewer than 20 bottles and consistently over for those consuming 20 or more bottles. Skog (1973) and Smith (1976) carefully reevaluated Ledermann's 1956 and 1964 work, comparing it with other studies, and showed exactly this tendency to underestimate the proportions in the higher consumption groups. At less than 10 liters annual consumption, the fit is poor by statistical tests. In estimating proportions of heavy drinkers, Ledermann predicts 2-2½ times the number of heavy drinkers in the 3-6 liter range compared with Skog's revised predictions (Smith 1976). Additionally, even at $m = 20$ liters, Ledermann's equation predicts 1.15 times the proportion of Skog's reformulations.

4. Differences in the distribution of consumption within populations has been either ignored, stated to be of no consequence, or stated to be quite small (ACP) and of no matter in much of the writings on the distribution-of-consumption model. This is a serious flaw in the application of Ledermann's concept. As Skog has shown, the lognormal assumption only works in homogeneous population groups and/or subgroups. Populations of heterogeneous background "show considerable systematic deviations from the lognormal distribution, and the deviations are always

the same; there are fewer heavy consumers than one might expect if the consumption had been lognormally distributed" (Skog 1973, p. 11). Clearly then, the variance as well as the mean of consumption is a very important parameter in determining the most appropriate distribution. The lognormal distribution *requires* these two parameters, the mean and the variance of consumption. Only in the special case of a population known to be homogeneous in drinking practices and culture can the variance be neglected and a good approximation to the consumption curve be obtained from the mean alone. Unfortunately, the variance is most difficult to obtain as production or sales data do not usually yield it, and that accounts for the appeal of Ledermann's "special case" in which the mean alone gives the curve.

5. Ledermann's work begins to identify major characteristics that affect consumption, such as age (the young drink more than the old) and sex (men drink more than women). This suggests that social patterns of drinking have some effect on consumption within particular groups; for example, the distribution of consumption for women might have a smaller mean and a larger variance than that for men.

Might not control policies need to recognize age-sex differences? An aggregate distribution might obscure these differences and thereby not permit a targeted control policy such as one designed to reduce the consumption of only the heaviest women drinkers while simultaneously it shifts most male drinkers leftward in the distribution to lower consumption levels.

In terms of policy, it is necessary to be very careful not to make an isomorphism between average consumption and alcoholism, particularly using only mean consumption as a parameter to determine the distribution. Heavy drinking may not equate to alcoholism on a one-to-one basis and should be more carefully explored, e.g., body weight and diet intervene between consumption and cirrhosis as does pattern of drinking, be it steady or episodic.

Overly simplistic models will produce results of less utility than necessary to the field. For example, the Ledermann model does not explain *why* drinking varies with age, nor even show that it might. In other words, we learn only relative proportions of light, moderate, and heavy drinkers without knowing subgroup variations until original data sets are partitioned, as Ledermann found necessary in his second and fifth samples, after having recognized differences in pattern of consumption. Ledermann's work does not provide much explanation, and at best offers only a crude predictive model.

Distribution theorists, though they acknowledge the growing methodological criticism, continue to defend the Ledermann proposition that there is a constant relation between mean consumption and the dispersion of the distribution (Schmidt and Popham 1978). They understand that without the Ledermann proposition, a cost-effective program of primary prevention is probably precluded because in one population the expenditure of millions might reduce mean consumption by 5 percent and reduce the proportion of heavy consumers by 10 percent, while in another population an equivalent reduction in mean consumption might be accompanied by only a 0.5 percent reduction in the proportion of heavy consumers.

Conceptual Limitations of the Model

Propositions 5 and 6—that the incidence of physical health problems can be reduced by lowering per capita consumption and that per capita consumption can be lowered by raising the price of alcohol relative to disposable income—were suggested by the work of Seeley (1960). Strong zero-order correlations between the cirrhosis mortality rate and per capita consumption and between per capita consumption and price in Canada were demonstrated. Assuming that below a certain level of licit consumption enough illicit consumption is made available to raise true total consumption, Seeley speculated that “If an attempt were made by a suitable government to change (in either direction) to a sufficient degree (say 20 percent to 30 percent) the already, in effect, administered price of alcohol for a sufficient period (say three years), it could be determined whether indeed licit alcohol consumption and, therewith, the rate of death by cirrhosis of the liver change in the expected direction and to the expected degree.”

In Seeley’s work and the distribution research that followed it, the only public health problem that consistently correlates with heavy consumption is liver cirrhosis, certainly a serious disease and one not to be minimized. However, other problems of physical and mental health occur that should be assessed in the design and evaluation of any prevention effort. In addition to knowing the effect of a price raise and consumption change on the cirrhosis rate, we would also like to determine the effects on the rates for hypertension, cancer, mental illness, traffic accidents, child abuse, and other problems.

Distribution theorists also have indicated a desire for data on these outcomes but nevertheless have used cirrhosis data as their

foundation for constructing a prevention model. Prior to formulating general propositions, however, there is a need for further research on relationships such as that between alcohol consumption and coronary heart disease (CHD). If it can be demonstrated that alcohol consumption and CHD are related, then research also is needed on the relation between relative price and CHD.

Previous research on the relation between alcohol consumption and CHD has been inconclusive. In an original analysis of the Framingham data, no association between alcohol consumption and myocardial infarction was found (Dawber et al. 1959), and in a secondary analysis a slight negative association was reported (Stason et al. 1976). A strong negative association between moderate alcohol consumption (up to 60 ml absolute alcohol per day) and the risk of nonfatal myocardial infarction and death from CHD was found in a recent study of 7,705 Japanese men living in Hawaii (Yano et al. 1977). However, data on male twin pairs in Sweden and the United States show that alcohol drinking and to a lesser extent occupational adjustment are related to angina pectoris directly and not through their association with other factors such as age, genetic background, smoking, physical exercise and early environment (Hrubec et al. 1976).

Of course many of these medical and social problems are difficult to measure, leaving no evidence such as fatty destruction of liver tissue visible at biopsy or autopsy of the cirrhotic drinker. Yet the cost to society of these problems is many times higher than the cost of caring for the cirrhotic alcoholics in moderate-to low-consumption countries. The costs are diffuse, often involving the health of others who are not alcohol abusers.

A question we hasten to raise is whether reducing per capita consumption through price control might not result in a change in drinking patterns from steady to episodic consumption, from greater frequency and total consumption to greater quantity consumed per occasion. We are asking specifically if a set of policies that might reduce a chronic problem such as cirrhosis might not increase an acute problem such as cognitive impairment. Put another way, what are the unanticipated consequences of the control policy recommended by distribution theorists?

That a particular drinking pattern, quantity per occasion, is related to a medical problem, cognitive impairment, is indicated in a recent study. Parker and Noble (1977) assessed the relationship between drinking patterns and cognitive functioning in a sample consisting primarily of social drinkers. One hundred and two males who were employed in managerial and professional occupations were given a battery of tests measuring abstracting and adaptive abilities. Their cognitive scores were considered in

relation to their lifetime consumption, current frequency and current quantity. Neither lifetime consumption nor current frequency of drinking was significantly related to cognitive performance. Current quantity per occasion, however, was consistently correlated in the direction of decreased performance with increased drinking per occasion.

The second point to be made here, then, is that certain alcohol-related problems appear to be associated with a particular drinking pattern and, as will be shown, with certain socioeconomic groupings. More generally, it would seem that it is necessary to introduce further variables in addition to per capita consumption into a predictive model of alcohol-related problems. In regard to the drinking pattern, Lelbach (1976) points out that a pattern of continuous drinking seems to be of greater danger to the liver than periodic or intermittent drinking, and he cites the high incidence of alcohol-induced cirrhosis in France which is associated with a pattern of regular daily consumption. Distribution theorists, however, imply that the pattern is largely irrelevant: "a high prevalence of 'chronic' alcohol problems is very unlikely to occur in the absence of a high prevalence of 'acute' problems; nor, for that matter, is the reverse likely to be found" (Popham et al. 1976). In other words, a low prevalence of "chronic" alcohol problems is thought likely to accompany a low prevalence of "acute" problems. At present, the evidence for this argument is somewhat less than convincing. For example, it was estimated that 28 percent of alcohol-impaired drivers in Ontario were "alcoholics in the clinical sense." If we may assume that this group consists of drinkers hospitalized or admitted for treatment, are we to assume as well that the remaining 72 percent were not heavy drinkers? If so, this is hardly strong support for the hypothesized conjunction of chronic and acute problems.

Some chronic problems are associated with certain social categories. In a recent epidemiological study that is notable for its use of modern analytic techniques, Mathews (1976) shows that for men aged 20-64 in England and Wales during 1949-1953, high administrators, other administrators, personal service and armed forces personnel, and to some extent clerical workers have higher standardized mortality ratios (SMRs) for cirrhosis, vascular lesions of the nervous system (stroke), and coronary heart disease than do other occupational groups. SMR, the ratio of actual deaths from a specific cause in an occupational group to the number to be expected if the age-specific death rate from that cause for the total population of working age had prevailed in that occupational group, is utilized in a multiple regression analysis. A single pre-

dictor variable, the SMR for cirrhosis by occupational group, accounts for 65 to 69 percent of the variance in SMR for hypertension, stroke, and CHD. However, when a much smaller subset of 15 occupational classifications with the highest cirrhosis mortality was examined, there were significantly fewer deaths than expected from CHD. The 15 specific occupations include alcoholic beverage makers, owners of wholesale businesses and retail food stores, Catholic priests and monks, judges and lawyers, medical doctors, army officers and enlisted personnel, and indoor servants. Following the infectious disease paradigm, Mathews speculates that "alcohol exposure" is greatest in the subset of 15 occupational classifications, suggesting that heavy alcohol consumption may be associated with a risk of CHD that is decreased relative to the increased risk of hypertension and stroke. Other models are possible, however, such as a linear, recursive model that traces the direct and indirect effects of occupational stress on cardiovascular disease.

That a model of occupational stress might be appropriate is indicated by the trend analysis by Brenner (1975) alluded to earlier. Brenner suggests that while wine and beer consumption, in general, reflect prosperous and stable periods as indicated by per capita income and the employment rate, distilled-spirits consumption reflects long-term prosperity and short-term economic stress. Brenner further suggests that these apparently opposite movements of per capita consumption of distilled spirits may reflect the activities of one population that uses alcohol during periods of prosperity and of another that uses alcohol to cope with conditions of stress. Put in sociological terms, one population may be nonmobile or upwardly mobile; the other, downwardly mobile. Upward mobility can be stressful, of course, as is downward mobility with job loss and decrease in personal income, which Brenner apparently wishes to focus upon. If both are stressful, though, it will be necessary to identify a population which is neither upwardly mobile nor downwardly mobile, presumably a less-stressed group.

The explanatory importance of such socioeconomic groupings is important to grasp for understanding the limits of proposition 6, the availability proposition, which we suspect is applicable only to upper segments of the middle class. Consider the broad formulation of it by Storm and Cutler, who hypothesize that "the greater the disposable resources left free by alternatives, the greater the consumption" (1975). Storm and Cutler's hypothesis is not supported by a number of empirical studies which report that lowest status groups and social classes, which possess the

fewest disposable resources such as income, also have the highest prevalence of heavy alcohol use.

Warkov and associates (1965) found that incidence of identification as a problem drinker varied inversely with social, occupational, and organizational status of employees. Schuckit and Gunderson (1974) reported that skilled or technical jobs appeared to offer lower risk for developing alcoholism than higher risk jobs such as service, sales, and semiskilled labor. However, specific conditions of work that increase the risk of certain status incumbents' becoming deviant drinkers have yet to be systematically studied.

Distribution theorists have questioned the validity of survey data (Popham 1970), and it is appropriate that they do so because one of the best national surveys does not support their position on availability. Cahalan and Room (1974) found that lowest status men have a much higher percentage of "steady fairly heavy drinkers," particularly in combination with "very heavy drinkers." (Note: The Cahalan and Room commentary in the text appears to reverse what is shown in their table). "Steady fairly heavy drinkers" are defined as drinking five or more drinks per occasion at least once a week and "very heavy drinkers" are defined as drinking twelve or more per occasion at least once a month. Age, though, is not controlled in this later analysis and if controlled the association between social position and heavy consumption might disappear.

Recent studies suggest that the *fewer* the disposable resources left free by alternatives, the greater the consumption. Following the tradition initiated by Horton (1943), Pearlin and Radabaugh (1976) reported that the consumption of alcohol is a mechanism for coping with anxiety aroused by economic conditions. They found that there was a significant inverse correlation between family income and anxiety and a significant direct relation between the level of anxiety and the use of alcohol for control of distress among a sample of 2,300 people representative of the Chicago metropolitan area.

In an unsystematic study with a small quota sample, Brisbane (1976) reported that black youths who come from poor socio-economic environments may drink abusively because of their inability to cope with social inequalities. Blacks, Brisbane claims, are situated in communities in which there is low employment, low educational attainment, low aspiration, and low self-esteem. In Brisbane's view, his survey confirmed other findings that, for the disadvantaged, alcohol provides a way to cope with the burden of poverty and "carry the added miseries of ethnic rejection."

In a report prepared by Medicine in the Public Interest (1976), it is suggested that alcohol consumption can be influenced by such availability factors as pricing and taxation policies, age restrictions on purchase, formulas for the number of on-premise and off-premise sales outlets, and regulations on the hours and days of sale. The NIAAA Prevention Division draft position paper also suggests that these factors may affect alcohol consumption. However, empirical research to support these speculations is needed.

Some Finnish research indicates that alcohol consumption varies directly with availability. A study of the liberalization of the number and location of alcohol outlets and the type of beverages marketed during 1968-1969 in Finland revealed that new drinking practices emerged but ones which did not moderate previously established habits; thus total consumption increased. Liberalization of control policies promoted an additive, not a substitutive effect (Mäkelä 1975). Further study (Sulkunen 1976) suggests that the diffusion of drinking cultures encouraged the convergence of consumption levels both in terms of quality (specific beverage consumed) and in terms of quantity (total alcohol consumed).

Other research suggests that changes in consumption are unrelated to availability. Treml (1975) found that during 1957-1972 in the Soviet Union, alcohol consumption was increasing at an average annual rate of approximately 5.6 percent, yet prices of standard brands of alcoholic beverages remained relatively stable. Information is required on the changes in disposable income during the period in question in order to assess the importance of these findings, but it is possible that the Soviet Union constitutes a negative case for the distribution model.

Some Canadian research suggests that alcohol consumption is unrelated to changes in availability. Smart and Finley (1976) studied the effect of changes in drinking age and per capita consumption in 10 Canadian provinces and found that reducing legal age limits had no consistent effect on beer sales. Advertising restraints also seem ineffectual. Advertising may heighten awareness of existing availability and so promote increases in per capita consumption, but Smart and Cutler (1976) examined the effects of a 14-month ban on the advertising of alcoholic beverages in British Columbia and found no support for the hypothesis that the ban had an effect on either yearly or monthly consumption of beer, wine, or liquor when compared with Ontario, which did not have such a ban.

When the distribution model does not appear to have a good fit to some data, population heterogeneity is bootlegged into the

explanation. For example, in a recent paper Popham and associates (1976) note that the Netherlands had high availability but low consumption and cirrhosis and suggest that this can be attributed to the prescriptive rules of the local culture among Calvinist segments of the population. At other times it is found that class heterogeneity as indicated by income is forced upon the analyst by the data. For example, Smart (1977), employing an eight-item index of availability, correlated availability scores for U.S. States with per capita consumption rates, alone and with average income and urbanism partialled out. The zero-order correlation was positive and significant; however, the partial correlation with income controlled was not significant, suggesting that income is a more likely determinant of per capita consumption. This is strongly indicated in a study of Irish beer and spirits consumption (Walsh and Walsh 1970). None of the coefficients for the price variables were significant, although they were in the expected negative direction. Income, however, was very highly correlated, accounting on its own for over 90 percent of the variance in the dependent variable.

ACP suggests that pricing and taxing policies might be appropriate but concedes that elasticities are not inherent attributes of alcoholic beverages. Vladeck and Weiss (1975) observe that there always remains the question of whether price and nonprice elasticities of demand for alcohol are uniform throughout the population or whether elasticity is much lower among the "abuser" tail of the lognormal distribution. That elasticities are not inherent properties of beverages is shown by a recent analysis of Canadian data.

Lau (1975) reports that during 1949-1969 in Canada spirits and wine were significantly price and income elastic but that beer was inelastic. Per capita adult consumption of beer is "entirely insensitive" to changes in its price such that a rise in the real price of beer will lead to an almost proportionate increase in consumer expenditure on beer. Consumption of spirits in Canada is also highly inelastic as has been demonstrated in a recent examination of Lau's regression equations (Lidman 1976).

We note in this connection that most analyses of consumption and recommendations for control focus on price as the measure of availability. This focus is explained when one recognizes that price increases benefit the alcohol producer and the State and are appropriate to laissez-faire capitalist economic assumptions, while restrictions on production output and sales cause organized complaints from powerful sources under this same economic assumption. In addition, a price policy is consistent with a psychogenic

focus on the individual as the source of problematic alcohol consumption, while a sociological standpoint suggests that there are structural roots of importance which distribution theorists usually ignore.

For an Expanded Prevention Model

In sum, the distribution-of-consumption model has serious limitations. Empirical support is weak, inconclusive, and even negative both for the Ledermann proposition that there is a constant relationship between mean consumption and the dispersion of the distribution and for the availability proposition that mean consumption can be lowered by raising the price of alcohol relative to disposable income. And the conceptual range of variables is overly restrictive.

We are interested in both prediction and explanation in the multivariate sense; that is, we want eventually to formulate equations that will include terms that collectively account for a high proportion of the variance and individually contribute significantly to the explained variance. Therefore, we believe that a viable prevention model should focus on a wider range of alcohol-related medical and social problems and structural and behavioral determinants of these problems. By relating these sets of predictor and criterion variables, we can answer questions such as the following: Does manipulation of price inversely affect all medical and social outcomes, or does price manipulation affect cirrhosis inversely, but cognitive impairment and other problems directly? By carrying out research that will facilitate the employment of what we call a "structural model," an aggregate cost-benefit analysis of alcohol-control policies is possible that can consider the trade-off of a reduction of cirrhosis for an increase in cognitive impairments and other problems that might be generated in part by a change from higher total consumption to lower total consumption with higher quantity consumed per occasion.

We cannot develop the structural model here, but we can indicate that it would incorporate variables from what have been called the distribution and socialization approaches to alcohol abuse and alcoholism (Whitehead 1975). Medicine problems such as cirrhosis and social problems such as marital discord would be predicted by both per capita consumption and patterns of drinking, and these variables in turn would be predicted both by alcohol availability and by heterogeneity of the population conceived in terms of ethnicity, social class, and other demographic character-

istics. The conduct of epidemiological and social survey studies and the use of path analytic techniques and other multivariate statistical procedures would be appropriate for elaboration and refinement of the model.

Our position, then, is a modest one. A program of primary prevention, of reducing the incidence of alcohol-related medical and social problems, requires a strong foundation in research. In introducing a prevention program, we need to know the values of a number of variables—not just availability but also heterogeneity; not just how availability affects per capita consumption, but also how it bears upon drinking patterns; not just how drinking patterns relate to medical problems, but also how they relate to social problems. At the point where we can assess these changes, we may be ready for massive social engineering. In the meantime, we should proceed cautiously, building a prevention effort on a solid base of research on the variables specified in the structural model outlined above.

Of what use then is Ledermann's work and that of those who followed him? His model provided a systematic approach to alcohol consumption and a valuable attempt to blend mathematics with the social sciences. Most evidence suggests that he chose the right family of curves for his model. The difficulties arose because of the assumptions made by others that Ledermann's work was in a final stage, rather than in an early and tentative stage of development as he himself indicated in his writings. Those who followed have helped by conducting an impressive number of studies of alcohol consumption. Their work has and will stimulate discussion and development of the understanding of the consumption of alcohol. It is time now to improve upon the data sources and conceptual framework so that a comprehensive test of a revised model will be possible.

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Discussion of Paper by Parker and Harman*

Wolfgang Schmidt and Robert E. Popham

Introduction

To arrive at a reasonably balanced assessment, particularly of Ledermann's contribution, but also that of later workers who have sought to develop his model further and explain its practical implications, it is important to see the distribution model in a historical context.

In the 1960's, when we first brought the work of Ledermann to the attention of researchers outside of France, two schools of thought relevant to prevention could be distinguished. One of these derived from the classical-disease concept of alcoholism and the associated view that alcohol problems are largely attributable to the behaviour of the alcoholic. From this standpoint, alcohol use per se is seen to be of little importance. The alcoholic is not simply an unusually heavy drinker but is believed to differ fundamentally from the social drinker—he suffers from a specific disease, and his heavy alcohol use is purely symptomatic. This position is clearly stated in a recent submission to Government prepared by the Association of Canadian Distillers (1973): "Alcohol and alcoholism are two entirely different subjects . . . Just as sugar is not the cause of diabetes, alcohol is not the cause of alcoholism."

During the post-war period, the emphasis on the treatment of alcoholics as the main remedial measure flowed logically from this view. Primary preventive approaches, other than perhaps early identification, were precluded on the grounds that alcoholism is an immanent condition, largely immune to environmental manipulation.

The second school of thought took as its point of departure the observation that in some European countries where alcohol is

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used regularly with meals and is an integral part of everyday activities, gross drunkenness and other types of dangerous drinking appear to be uncommon. Accordingly, it was felt that prevention could be achieved by encouraging drinking as an incidental part of routine activities. To facilitate such integrative drinking, this school of thought has advocated the adoption of customs such as the use of wine with meals and has sought in various ways to raise the acceptance of alcohol use in society. It is reasoned that more problems result from the mysticism associated with alcohol and from guilt attendant upon its use than would occur if drinking were an accepted part of everyday life. To aid in the demystification process, it has been recommended that restrictions on the advertising of alcoholic beverages be removed and that the legal drinking age be lowered and alcoholic beverages introduced at an early age. Generally, legal restrictions are seen as impediments to the adoption of healthy drinking styles and, therefore, as undesirable.

The shortcomings of these two approaches have been discussed at length elsewhere (Popham et al. 1976; Schmidt 1977b) and need not be reiterated here. The crucial point is that both schools denied any role to the overall level of consumption in a population as a determinant of the prevalence of heavy use. It is, therefore, not surprising that Ledermann's work was largely ignored in his day. He had indicated that the most important result of his research was "the finding of a 'quasi-mathematical' connection between moderate and heavy consumption" and suggested that this left "little hope for those who want to reduce alcoholism without affecting the [overall] consumption" (Ledermann 1965 p. 8).

The attitudes that prevailed in the 1960's have by no means disappeared, and they seem to us to account in part for the nature of some of the attacks to which his work has been subjected in the last two or three years. But we shall return to this point in our concluding remarks. Let us turn now to the critique at hand. In our view, the following propositions relevant to the issue of prevention may be derived from Ledermann's work:

1. A change in the average consumption of alcohol in a population is likely to be accompanied by a change in the same direction in the proportion of heavy consumers.
2. Because heavy use of alcohol generally increases the probability of physical and social damage, average consumption should be closely related to the prevalence of such damage in any population.

3. Any measures that may be expected to affect overall consumption, such as those regulating the availability of alcohol, are likely also to affect the prevalence of alcohol problems and should thus be a central consideration in any program of prevention.

In responding to the critique of Parker and Harman, it appears that it will be important to assess the extent to which any of their points seriously challenge the validity of these three propositions, which we believe are basic to the so-called single-distribution model of prevention.

The Supporting Data for the Ledermann Distribution

To study the distribution pattern in detail requires a breakdown of alcohol consumers in a given population according to the quantity consumed within a specified period. Such data are only occasionally reported in the literature, and Ledermann used what little he could find. He usually gave only a brief description of the data he employed, but provided the full references to the original reports.

Parker and Harman reject Ledermann's hypothesis largely on the ground of assumed shortcomings in the supporting data he used in his 1956 report. However, the problem lies partly in their failure to consult the original reports from which the data were obtained. We believe that, had they done so, in most instances they would have been led to rather different conclusions. For example, they follow Smith in assuming that the Chicago sample comprised accident drivers or traffic violators. In fact, the data were those collected in the classic study by Holcomb (1938) and represent the blood-alcohol level found in his control group of non-accident drivers who were stopped at random for the purposes of the research.

Rather more important is the failure of the critics to examine Ledermann's subsequent work. For example, in 1964 he added the Bresard survey data from St. Étienne and the control samples in the Pequignot and Schwartz morbidity studies. Again, the samples and methods of study have been described in great detail by these authors. Bresard's survey is unique in that the self-report consumption data agree very closely with estimates based on official sales figures. The Pequignot data are exemplary for the detail achieved in the drinking histories.

Parker and Harman also fail to take into account the recent work of subsequent investigators. For example, Bruun et al.

(1976) analyzed the data of 12 additional surveys from five European countries. By and large, these support Ledermann's contention concerning the close relationship between the mean and heavy consumption. To this already extensive series must be added the de Lint and Schmidt (1968) documentary data relating to alcohol buying in Ontario and the data concerning the blood-alcohol levels in the control group utilized in Borkenstein's Grand Rapids Study (Silberfeld 1972), both of which were remarkably well described by the Ledermann theoretical distribution. All in all, we are led not only to reject the contention that the data are insufficient to support the central hypothesis, but we are unable to think of another proposition in the alcohol field in support of which there is such a wealth of evidence.

The Constants in the Ledermann Equation

This conclusion does not imply that the quantity, θ , as derived from such samples in Ledermann's first approximation, is a constant in the strict sense of the term. As Ledermann pointed out, θ can only be a rough approximation, and it must be realized that the relationship described is statistical, that is, it involves variation around central tendencies. Furthermore, there is good theoretical reason to believe that the model may systematically underestimate or overestimate prevalences. Thus, we agree with Parker and Harman that the distribution model can only provide an approximation, and with Skog's contention that "neither the dispersion nor the number of heavy consumers in a population can be uniquely determined by the mean consumption" (1977, p. 11). However, we also concur with Skog's concluding remark on the issue that "for most practical purposes, a simple one-parametric distribution (such as the Ledermann-distribution) is likely to give a fairly good approximation." The crucial question, then, is how much variation in dispersion is compatible with the proposition that the proportion of heavy drinkers varies in the same direction as the mean.

For example, a change in dispersion may counterbalance a change in the mean so that an increase in overall consumption may have no effect on the proportion of heavy drinkers. After a thorough review of the evidence on this point, Bruun et al. (1976) concluded that the differences in dispersion among the populations so far examined are quite small and probably of little practical relevance. It is regrettable that Parker and Harman—and others before them (e.g., Miller and Agnew, 1974; Smith 1976)—have been preoccupied with the shortcomings of the Ledermann equa-

tion as a device to obtain specific estimates of prevalence. Certainly the variation in dispersion will increase the range of error of such estimates. The point is simply that this error is not sufficiently large to seriously threaten the central proposition of the model that average consumption and heavy consumption covary.

Concerning the constant D , we agree that the maximum consumption of 365 liters of absolute alcohol a year—which is the value Ledermann assigned to D —constitutes a humanly impossible level of use. Experimental work, such as that of Newman, Isbell, and, most recently, Mendelson, would provide a valid base for the estimation of a realistic maximum.

According to Parker and Harman, there are additional points which have a direct bearing on the validity of the single-distribution notion. For example, they suggest that the aggregate distribution may disguise nuances in the distribution patterns of subpopulations, that heavy drinking may not equate with alcoholism, and that the Ledermann model may not explain why drinking varies with age. One can only agree with these points, but it is difficult to see their relevance to the model of prevention based on the distribution. Concerning the masking of subpopulation drinking patterns, such data are obtainable by surveys and other means, and it is obvious that the distribution model does not explain *why* a person or a group drinks as they do. The model at best provides an approximation to the proportion of drinkers at various consumption levels, once it is known what quantity of alcohol is being placed at the disposal of a population. On the issue of equating heavy drinking with alcoholism, we have generally tried to avoid the use of the latter term. Alcoholism seems to us an ill-defined label for a variety of symptoms and behaviours. To circumvent the vagaries involved, we have defined heavy use on a strictly quantitative basis.

In their final assessment, Parker and Harman claim that, because the relation between mean and the measure of dispersion is not constant and because the distribution is not necessarily log-normal, the Ledermann proposition is not redeemable.

At the risk of becoming repetitive, one can only emphasize again that constancy in dispersion, or in the relationship between mean and dispersion, is not a prerequisite. What is required is an absence of *major* variation in the measure of dispersion among populations. As we have pointed out, there is a wealth of evidence that indicates that this condition is met. Secondly, as Skog (1977) has argued, it is not essential whether the distribution belongs to the lognormal family, the gamma family, or some other class of distribution. The crucial point is that the distribution is approximately one-parametric and, on this issue, there exists a great deal

of supportive evidence from a large number of populations. Thus the Ledermann proposition, as formulated in Bruun et al. (1976), is not only redeemable but is in our view quite indisputable.

Cirrhosis Mortality as an Indicator of Alcohol Problems

Parker and Harman feel that in addition to knowing the effect of a change in consumption on the cirrhosis mortality rate, one should also determine the effects on other health and social problems.

It must be emphasized that the cirrhosis death rate is employed by us because of its particular value as an indicator of heavy consumption and not because it is by any means the only important consequence of heavy drinking. Alcohol use is also a factor in the etiology of gastritis, pancreatitis, cardiomyopathy, peripheral neuropathy, and toxic psychosis. Alcohol use is definitely related to cancer of the mouth, pharynx, larynx, and esophagus and is a major factor in suicide, assault, and accidents. This is to say nothing of its impact on industry through reduced productivity and absenteeism and its role in family breakdown and child neglect. But only cirrhosis has proved to be a fully reliable index of the magnitude of alcohol problems in a population. We have discussed this issue at some length elsewhere and have described under what conditions one would expect to find correlations between the rate of a problem and the level of alcohol use in a population (Schmidt and Popham 1974). In the main, the issue centers on the size of the portion of a particular condition that can be attributed to alcohol, the question of multiple etiology, and the magnitude of the problem. After a systematic examination of several alcohol-related morbid conditions, we concluded that only cirrhosis has the attributes that make it suitable as a general index of the magnitude of chronic alcohol problems.

In other words, an increase in deaths from cirrhosis in a given year implies that, at some previous time, the number of chronic heavy users of alcohol increased proportionately. Because consumption patterns that are conducive to the development of cirrhosis also result in a wide range of other health and socioeconomic problems, it is a reasonable assumption that changes in cirrhosis mortality reflect changes in all problems resulting from the chronic heavy use of alcohol.

Parker and Harman also suggest that the cost of caring for the

cirrhotic is comparatively low in moderate consumption countries. It is instructive in this respect that in New York State and Ontario, for example, cirrhosis is now the third leading cause of death after heart disease and cancer among males in the productive years of life between 24 and 65. In Ontario and some parts of the United States, this disease has been the most rapidly increasing cause of death for some time, and more than 80 percent of all cirrhosis deaths in these jurisdictions are now attributable to alcohol use (Schmidt 1976, 1977a, 1977b). Certainly the cost cannot be negligible, and the trends are ominous.

Concerning the role of drinking patterns in the development of cirrhosis, Parker and Harman have apparently misunderstood Lelbach. He demonstrated convincingly that it is the volume consumed over the drinking life rather than the pattern of consumption—periodic or intermittent—that determines the risk of cirrhosis. It just happens that steady drinkers tend to consume more in the long run.

When commenting on problems of physical health, the critics suggest that cirrhosis may have a protective function with respect to some forms of cardiovascular disease. This is a highly controversial issue and cannot be resolved by epidemiological methods. Similarly, the findings that occupational groups with the highest cirrhosis mortality show a risk of CHD that has decreased relative to the increased risk of hypertension and stroke are quite irrelevant in a public health context. The important observation from a preventive point of view is that the occupations with the highest cirrhosis mortality also have a high overall mortality, exceeding the expectations based on general populations. This is not to say that there are no partially compensatory decreases in some causes. For example, alcoholics have a higher than expected rate of death from all cancers combined, but a lower than expected rate from some cancers. However, this may not imply a protective action of alcohol. It may indicate that alcohol in some instances affects the site rather than the risk of occurrence of malignancies.

In sum, we do not share Parker and Harman's belief that increases either in individual or in population consumption levels—no matter how they are brought about—may involve compensatory beneficial effects. Nor do we share their concern that decreases are likely to produce a significant rise in the prevalence of new problems through a shift toward more destructive patterns of use. There is simply no evidence to support such predictions. In any case, we do not recommend the adoption of measures intended to *reduce* consumption. Rather, in our recent proposal to the Government of Ontario, the objective was the *stabilization* of

the consumption trends (ARF 1973). Alcohol use in Canada and many other parts of the developed world has been increasing at a very rapid rate in the recent past. Our aim is to prevent further increases; therefore it might have been more appropriate if Parker and Harman had weighed the possible gains and losses of a continued rise in consumption as against a stabilization of the trend. This would put the matter in a more realistic light than do their speculations about the possible effects of some drastic impositions—economic or otherwise—on the availability of alcohol.

The Availability Proposition

Parker and Harman suggest that the “availability proposition” is applicable only to upper segments of the middle class. They question Storm and Cutler’s (1975) formulation that “the greater the disposable resources left free by alternatives, the greater the consumption” and offer empirical evidence that lowest status groups have the highest prevalence of heavy drinking. The error here lies in the use of a global proposition to explain differences between socioeconomic subgroupings within the same population. Thus, for example, the class gradient in the prevalence of alcohol problems in Ireland and the United States is probably very similar. However, the fact remains that in the former the prevalence of alcohol problems in the working classes (and for that matter in the country as a whole) is much lower than in the United States. This appears to be due to a lower economic accessibility in Ireland (Walsh and Walsh 1970). Certainly, on the basis of the studies of Bales and others, one could not attribute it to a high degree of dry sentiment in Ireland. In any case, the consequences for the drinking problems of emigrants were very revealing. Working class Irish in the United States have consistently ranked highest on a variety of indices of alcohol problems (Bales 1946). It is not unreasonable to suggest that this was at least partly attributable to a high-acceptance group finding itself in a more favorable position with respect to economic accessibility.

The reference to Pearlin and Radabaugh (1976) concerning reliance of the poor on alcohol as a distress management device is likewise inappropriate in the present context. Let us assume, for the sake of argument, that Pearlin and Radabaugh are correct—low-income families have high rates of anxiety that cause high rates of alcohol use and attendant problems. Certainly this is hardly a startling proposition. But can we not assume with Edwards (1971) that the *amount* of this reliance will be greater

where alcohol is inexpensive and readily available and where attitudes toward drinking are permissive? Surely the level of economically induced anxiety will be as high or higher among low-income people in India, Egypt, or Ireland. But in these countries, drinking problems are either nonexistent or much less prevalent than in the United States.

With regard to the empirical basis of the availability proposition, Parker and Harman state that much current research indicates that availability varies directly with consumption. We agree. Parker and Harman cite the recent Finnish study (Bruun et al. 1976), in which vast increases in consumption, occasions for drinking alcohol, and the number of heavy drinkers were found following a dramatic increase in availability. The critics then offer evidence which, in their view, suggests that at least some changes in consumption may not be related to availability. Although we would not question *a priori* that such unrelated changes may occur, we do question the data offered in support of this view by Parker and Harman.

There is the case of Russia, where consumption is said to have increased at an astronomical rate despite a stable price situation. Firstly, the rate of increase is not, as claimed, "one of the highest." For example, the increases in Holland, West Germany, and Austria were considerably greater over the period in question. Secondly, many reports indicate that the living standard in Russia has steadily risen, which implies that the real price has declined. Furthermore, it must be remembered that the post-war years in the U.S.S.R. were a period of a great shortage of consumer goods, including alcoholic beverages, a situation which has steadily improved since. No doubt all these factors have contributed to the rise in consumption.

Our explanation of the comparatively low rate of consumption in Holland was labeled "bootlegged heterogeneity" by our critics. In fact, we discussed the case of Holland to illustrate that economic accessibility certainly does not always explain the variation found and that the level of acceptance was undoubtedly another very important factor. In this connection, it is instructive to consider recent trends in alcohol consumption in this country. Over the last decade or so, the influence of the rural regions of the country, where Calvinist sentiments were strong, has been greatly weakened, particularly since the advent of the European Common Market, with the subsequent rise in affluence and cosmopolitanism. Concurrently, the Netherlands experienced a three-fold increase in consumption, the largest rate of increase over the period of which we are aware. The Dutch increasingly have adopted

French drinking styles including the consumption of wine, which became very inexpensive under Common Market arrangements. All in all, the developments in Holland are consistent with the availability proposition.

Other aspects of availability examined by Parker and Harman include reductions in the legal drinking age, advertising, and the effects of price and income. First, the report of Smart and Finley (1976) is cited to indicate that reducing the legal drinking age had no effect on beer sales in Canadian provinces. However, this study has to be interpreted with caution because the data did not permit a before-and-after comparison of drinking in the age groups affected. In fact, Smart and Finley utilized overall per capita consumption data on the grounds that a change in drinking among the young—if of sufficient magnitude—would be reflected by this aggregate measure. The question then becomes: How large an increase in consumption among young drinkers would have been required to measurably affect per capita figures for the general population? Consider that the age group in question represented about 5 percent of the potential drinking population. Thus it would have required a doubling of the group's consumption to produce a 5 percent increase in overall per capita consumption, and then only if the group were already drinking at the level of the adult population prior to the change in age limit. The latter seems highly improbable. One may also add that even an increase of 5 percent might be obscured by the chance fluctuations to which per capita figures are liable. In short, an almost unbelievably large increase in the beer consumption of the group would have to have occurred to have been detected by Smart and Finley's analysis, and indeed, these workers were aware that their approach did not permit rejection of the possibility that a significant increase had occurred.

Other work using more sensitive techniques of analysis has indicated that in Ontario, on-premises beer consumption increased markedly as a result of reducing the legal drinking age in the Province (Schmidt 1972; Smart and Schmidt 1975). Furthermore, in several independent studies of North American jurisdictions in which drinking age had been lowered, it was found that alcohol-related motor-vehicle accidents in the age group affected increased after the inception of the new law (Schmidt and Kornaczewski 1975; Williams et al. 1975; Whitehead et al. 1975). It could be established convincingly that these increases constituted an addition to the total accident involvement of the age group that would not have occurred were it not for the lowering of the legal drinking age.

Turning to the question of advertising, Parker and Harman cite Smart and Cutler (1976) as having found that a ban on the advertising of alcoholic beverages in British Columbia had no effect on consumption. But it has to be considered that the ban was not effective with respect to some advertising originating outside the Province. Furthermore, advertising specialists generally accept that a considerable time lag exists between a reduction in demand and the cessation of advertising, particularly if the population has been exposed to such advertising for many years. A 14-month period is certainly too short for a possible effect to become apparent.

Parker and Harman challenge the potential value of maintaining price to control the economic accessibility of alcoholic beverages and hence the level of consumption in a population, mainly on three grounds: (1) alcohol may be price-inelastic for heavy drinkers; (2) one or another class of alcoholic beverage may be price-inelastic; and (3) the influence of income has been given insufficient attention by proponents of price control.

On the possible differential effect of price on light and heavy consumers, Bruun and associates suggest that "the fairly consistent distribution of alcohol consumption in different cultures probably means that the regulation of total consumption through pricing-policy also influences the amounts consumed by heavy drinkers and thereby the magnitude of the harm done by alcohol. The close inverse relationship between changes in relative price of alcohol and variation in cirrhosis mortality observed in many temporal and regional comparisons is also consistent with this view" (1976, pp. 77-78). We may add the experience of Denmark after a major increase in the price of distilled spirits, then the preferred beverage of Danes. Not only did the overall consumption of alcohol greatly decrease, but consequences of heavy use such as death from delirium tremens and cirrhosis also declined sharply within a very short time. But let us assume for the sake of discussion that persons labelled alcoholic are incapable of controlling their alcohol consumption and would therefore be unaffected by price. Is not the principal aim of a preventive measure to affect indices rather than prevalence? As we have pointed out elsewhere (Popham et al. 1976), because the death rate of alcoholics greatly exceeds that of the general population, natural attrition alone would rapidly diminish prevalence if the inflow of new cases could be reduced. It is here that price control might be expected to have its most significant effect.

On the issue of beverage-class differences, Parker and Harman suggest that elasticities are not inherent properties of beverages and note that, in Lau's study of Canadian trends (1976), spirits

and wine were found to be price-and-income elastic but beer proved to be inelastic. However, Lau also reported that the three major classes of beverage may serve as substitutes for one another. Thus, cross-elasticities have to be taken into account in the formulation of price policies. For example, an increase in the price of spirits may lead to an increase in demand for beer. Although beer appears to be inelastic, such a shift in demand might be prevented through price adjustments. An important consideration in our work has been to point to the potential value of price as a preventive tool. Needless to say, specific recommendations must take local and historical conditions into account. Inelasticities and cross-elasticities are examples of these conditions. It is quite clear that recommendations as to how price policies should be formulated cannot be at once specific and universally applicable. Parenthetically, the statement made in this context that Canadians drink more beer than spirits and wine combined is no longer true. The per capita consumption of beer has remained quite stable while that of the other two classes has increased at a steady and often rapid rate. Consequently, beer now contributes about 40 percent to total consumption, and a long-term forecast indicates that by 1984 beer may contribute as little as 30 percent.

There remains the issue of income as an explanation of variation in consumption. We do not question for a moment that income is a very important determinant of demand for alcohol. But, as pointed out by Bruun and associates (1976), price manipulation may counteract the effects of increasing affluence. That is, under conditions of rising income, price increases may not produce a reduction but may limit the income-induced rise in demand.

The Validity of Survey Data

Finally, a word must be said respecting the implication that we have questioned the validity of survey data because such data did not support our position. In the first place, many surveys *have* provided support, for example, those summarized by Skog in the Bruun et al. (1976) report, the Bresard (1958) survey of the St. Etienne region, and Mäkelä's surveys (1971) before and after the increase in availability of alcohol in Finland. In the second place, we are only disillusioned with one specific shortcoming, namely, the apparent inability of the survey method to elicit—at least in North American areas—accurate information on the volume of consumption. This first became apparent to us in the course of our own survey work in Ontario. Thus we found that the total volume

of intake as reported in the survey constituted only a fraction of the volume indicated by sales data. Moreover, the amount of underreporting apparently was not a linear function of the volume of intake but tended to increase with level of use. Hence the approach did not offer a satisfactory way to test the accuracy of estimates of the rate of heavy use as predicted from the mean. On the other hand, with rare exceptions, survey data have supported the more general proposition that there is, in fact, a close relationship between the two.

Concluding Remarks

We do not want to leave the impression that we find no fault whatever with Ledermann's work on the distribution model. It is clear that there are deficiencies, especially in his derivation of so-called constants. But despite these shortcomings, the central element in his concept has not been seriously threatened, in our opinion, by the technical flaws and mathematical niceties of curve fitting on which Parker and Harman (and for that matter, other recent critics) have focused almost exclusively. In fact, the evidence accumulated since Ledermann's 1956 report has greatly strengthened the main proposition: the mean and the rate of heavy use are closely related. The danger is that in the enumeration of relatively minor flaws, the baby will be ejected with the bath water. We concur with Skog's recent assessment: "Ledermann's theory of the distribution of alcohol consumption is a remarkable example of scientific intuition. From a very modest data base, he reached conclusions which later studies have supported as essentially sound (1977, p. 4). It is perhaps well to recall that recent re-examination of Gregor Mendel's data has shown the improbability of the exact ratios he perceived in his experimental results. But no one has considered this to be grounds on which to challenge the essential validity of his concept of the nature of inheritance.

In defense of ourselves and others who have contributed to the single-distribution model, let us recall again the climate of ideas that prevailed some 10 years ago. The dominant view found among social scientists in the field as well as nonresearchers favored increasing the availability of alcohol and encouraging the adoption of drinking styles modeled especially on those of Italy and France. It is important to realize that this approach appealed to diverse groups which otherwise sometimes had conflicting interests: to the alcoholic beverage industry because no con-

straints on the volume of sales were advocated; to legislators because they could see scientific legitimization for what they believed to be a politically opportune approach, notably, liberalization of the control system; and to many others because it seemed consistent with the thrust toward increasing self-determination in matters of personal behavior.

But to a few of us—researchers with an epidemiological perspective—it was apparent that there were large and important discrepancies between the assumptions underlying the approach and events in the real world. Italian and French researchers (Ledermann among them) on the vast health costs attributable to alcohol use in their countries. Moreover, studies of the variation in liver-cirrhosis mortality indicated that one could not continue to ignore overall consumption levels with impunity. It seemed to us that North American researchers, in particular, had been misled by an assumption that what was acute and highly visible was a valid indicator of the prevalence of alcohol problems. The most familiar example is the image of Ireland as a nation of drunks. This perception rested mainly on the observations of Bales and others that special occasions typically involved drinking to intoxication. It overlooked the fact that these occasions were comparatively infrequent, and national indices of alcohol problems persistently indicated a low level of prevalence. In contrast, the visitor to France and Italy had no difficulty observing a great deal of drinking and was impressed by the apparent absence of drunkenness. The conclusion commonly drawn—by a key member of the Ontario cabinet among others—was that a permissive approach to alcohol leads to fewer problems. Needless to say, such visitors did not tour the hospitals nor did they include vital statistics reports in their holiday reading.

Lastly, we became aware of the rapid rise in alcohol consumption that was occurring during this period and has continued since, and of the pandemic character of the trend. Ledermann's work, some of Jellinek's international observations, and our own studies of trends in liver-cirrhosis mortality made at least one point abundantly clear: The overall level of consumption as a reflection of the size of the alcohol-related health bill in a population could not be ignored as a focus of preventive effort. Seeley's early work (1960)—stimulated at the time mainly by his interest in accounting for variation in liver-cirrhosis rates—suggested a possible method of prevention, namely, price control.

In our subsequent work, we may be justly accused of some overstatement and oversimplification. To a degree this was due to a deliberate strategy to secure a hearing for a point of view that

ran counter to the prevailing sentiment. In retrospect, we now doubt the effectiveness of such a strategy and are inclined to take a rather more conservative approach. In this respect, it may be useful to conclude by stating the three recommendations we recently made to the Government of Ontario as the basis for a comprehensive health-oriented alcohol control policy. Clearly the aims of these recommendations are modest and, in our view, take account of current trends and rest on a reasonable assessment of the evidence. The recommendations are as follows:

1. A taxation policy that maintains a reasonably constant relationship between the price of alcohol and levels of disposable income (income after taxes). For example, if disposable income per capita rose 5 percent in a year, then the price of each alcoholic beverage offered for sale would be increased by that percentage.
2. A moratorium on further relaxation of alcohol control measures and the adoption of a health-oriented policy with respect to such measures. Essentially, this would mean that future proposals to change legislative or other provisions governing the marketing and distribution of alcoholic beverages would be tested against a health objective: the prevention of further increases in the prevalence of alcohol problems. The relevant question would become: Are the proposed changes likely to contribute to higher consumption levels and therefore to an increase in health costs?
3. An education program designed to increase public awareness of the personal hazards of heavy alcohol consumption, the economic and other consequences for society of high consumption levels, and the potential public health benefits of appropriate control measures.

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General Discussion

Douglas Parker: I'd like to make some brief points. First of all, it is clear from Dr. Schmidt's response that he and Dr. Popham are establishing their own version of the distribution position. That is, they are not endorsing all expressions of that position that have been made by others, such as de Lint. For example, today they say they want to stabilize rises in consumption, but de Lint has said, "The first priority," and I'm quoting directly, "in government control measures should always be to reduce or stabilize availability." So there will be some need in the discussion to realize that we were addressing a somewhat broader position and that Popham and Schmidt have qualified that position and adopted a variant of their own. However, that does not keep them from citing such works as Bruun's book on *Alcohol Control Policies*, which they had a large role in writing, and simultaneously publishing almost identical statements to those that appear in that book.

I think there's a certain degree of misunderstanding on both sides. An example of the misunderstanding on the other side is that they misunderstand our position on coronary heart disease. We do not say that it has a protective function, we simply review some of the evidence that is inconclusive. To quote this one paragraph from our paper, we say "Previous research on the relation between consumption and CHD has been inconclusive." It is beyond me how that statement can be misinterpreted to imply support for the notion that alcohol has a protective function. The research is not yet in.

A third point is that we think they misunderstand Storm's proposition. It was intended to apply to comparisons within a population. Storm and Cutler talk about differences between occupational positions. They are not talking about the lower class in Ireland versus the lower class in the United States looked at in terms of their absolute level of alcohol consumption. It's a proposition that would ask for relative comparisons between the lower class in Ireland and, say, the middle and upper classes in Ireland and the lower class in the United States and the middle and upper classes in the United States.

They question the excellent secondary analysis by Tremel on the Soviet Union and say that there are reports that would indicate there is a rising standard of living in the U.S.S.R. I'm sure that Tremel would be interested to know what those reports are, because they're not cited. We realize that there is a certain lack of information, and we do not state unequivocally that the Soviet

Union constitutes a negative case. We simply suggest that it is a possibility that it constitutes a negative case. Also, in that connection, a point is made that there was earlier a shortage of alcoholic beverages, or a shortage of commodities, within the Soviet Union. This cannot be equated with whether commodities have a high price tag. You can have an abundance of commodities, but if they have a high price tag, that may restrict the availability. The question of shortage is another issue.

Finally, we do not want to return to the naive positions of the past—to the mythology of responsible drinking, the notion that somehow ethnic experience can be taken and transplanted to the national level. And I don't know if you intended to associate us with those previous positions or not. We are indebted to the distribution theory for helping to make a break with that tradition, and we want to build on it, we want to go beyond it. We do not, in any sense, want to return to the kind of loose, nonquantitative sort of speculation that was characteristic of the 1960's.

Marsha Harman: Our basic view on the univariate distribution differs from that of our colleagues, Wolf Schmidt and Bob Popham. A quantity appears large to us and small to them, for example, the error of approximation of the curve to known data and variation of the standard deviation of consumption. They equate BAC distributions with consumption distributions; we find fault with this practice of combining both kinds of data into the derivation of a distribution of consumption alone. We each selectively use the work of Skog to justify our own positions. Popham and Schmidt claim that we argue *for* beneficial effects of increased consumption when, in fact, we argue that decreased availability through pricing may simply bring about increase in one or more sets of problems (e.g., cognitive functioning) as a latent consequence, while fulfilling the manifest goal of reducing cirrhosis. We argue then for the kinds of revision in Ledermann's work that Drs. Schmidt and Popham have now begun in their rejoinder, with the three restated and substantially modified propositions. In particular, there is a need to expand and refine Ledermann's work in order to achieve better description and prediction and to begin explanation.

Control assumptions and policy recommendations of distribution of consumption theory that focus on availability in the current form are compatible with most political systems. This is a benefit of the theory. It provides workable, acceptable policy from some points of view. Using price to control availability is particularly compatible with North American political and economic institutions. Control over production would be less ac-

ceptable and more difficult to achieve in a profit-oriented economy. Any view of alcohol problems as endemic to specific social systems, to inequalities, to economic and opportunity injustices would be highly unpopular with governments whose interest is to preserve the status quo. Policy recommendations stemming from this perhaps radical view would be unacceptable to many governments. The price-control recommendation to limit availability does not challenge the assumptions of a profit economy and is effectively maintaining the status quo with the exception that less alcohol is available, possibly even increasing profits, and is expected to be acceptable to most laissez faire capitalist governments.

From the macroanalytic point of view this is a serious deficiency. It seems to say, "Let us not be so concerned with why people drink, but let us focus on making them drink less." This is an explanatory defect in the distribution-of-consumption theory. A theory is expected both to predict accurately and to explain adequately. Earlier, we showed predictive deficiencies of the Ledermann model when used unaltered. Now we must point to the failure to explain alcohol consumption as discussed by Doug. In its current form, the theory fails to fulfill either criteria. However, we find we can agree with neither the sociocultural nor the distribution theorists in their most orthodox conceptions nor even as they currently stand. Each taken alone lacks important explanatory and/or predictive power.

We have proposed a synthesis using variables such as population heterogeneity, availability of alcohol widely conceived, and drinking patterns. We feel that it is a first attempt to systematically unite the best part from each approach. The model is by no means complete. We have not accounted for microlevel variables such as role models, learning, and life crises, nor for demographic variables other than those that are components of socioeconomic and ethnic or cultural heterogeneity. Here is where the work of the Jessors and others needs to be selectively integrated. We feel that selected application of this or an augmented model will provide a better approximation to the understanding of the determinants, mechanisms, and consequences of alcohol use and abuse.

Thomas Storm: There are two relevant points that I thought were misunderstood in the paper by Parker and Harman. First of all, by resources, we did not mean simply income. To a large extent that weakened our position because it made it essentially irrefutable. We have to consider a lot of different kinds of resources. In my own mind, to some extent, the critical resource is time. And of course, the unemployed have a great deal of time available for drinking.

The other thing I think might have been a misunderstanding was that we do not really mean to apply the analysis to broad social factors in terms of income, nor to such broad categories as the white collar or the educational categories, but to specific occupational subjects within the conventional lower, middle, and upper classes that we felt might vary just as much *within* those broad classes as *between* those classes. And the availability of time and the structure of the occupation would determine the amount of time and opportunity for drinking activities and alcohol consumption. So I think that those things are clarified, and I don't really directly see the applicability of those points to the issue.

Reginald Smart: It seems to me that meetings of this sort involve a good many quarrels among different researchers over very small points, really very tiny details. I think there is a tendency to lose the major points. The extent of these quarrels is very much at variance with the kind of discussion that actually goes on when alcohol-control policies are considered and changed by legislators. Alcohol-control policies are changed for almost no reason at all—because legislators think it's a good idea or in response to public opinion, because they'd like to see it changed, or because somebody had lobbied them.

It seems that with the distribution approach we have an enormous amount of data far, far in excess of what is available on almost any other level that would help us to influence legislators. It seems that the data are certainly strong enough to generate an experimental trial somewhere. What we're really short of in much of this is data that come out of experimental trials. We're short of situations in which there have been major decreases in consumption without something like a war or prohibition. We very often argue about what would happen if there were major decreases in availability or there were major decreases in consumption, without really knowing what would happen. I do think that perhaps we should consider more about what are the major elements on which there could be agreement, rather than what is the large set of trivial elements on which there isn't much agreement. I think that concentrating on constants—whether a constant is really constant, how constant it ought to be—is very largely a waste of time. And I think it tends to generate a feeling on the part of legislators that scientists really just sit in rooms in hotels and quibble about small things, and never really get down to the basics or come to an agreement about anything.

Harvey Brenner: The question was raised as to whether, in fact, we don't have similar economic distributions across the European countries that are equivalent to the United States. In which case, why haven't we found more variability there

than we might have in the United States? There is a possible answer to that. It is entirely possible that we have a different character of lower socioeconomic status in the United States than we see in European society. A clear indication of this, even by National Labor Organization standards, is the considerably higher unemployment rate that is tolerated in the United States than is tolerated in European countries. It is entirely possible that matters related to relatively low income or in conjunction with low income, the status of unemployment and matters related to that status, whether they are the time variety, or the variety of shock, trauma, or loss, remains to be researched.

In any case, the character of socioeconomic status in the United States, by the economic status we have, is quite considerably different from that which prevails in European societies. In the study in Congress during this last year comparing the United States, Wales, and Sweden on mortality rates, there were indeed changes according to time and inflation. The same general formula or equation prevailed in the free countries. That is to say, a combination of those variables explained a great majority of the loss of the variance in the trends of mortality. Yet, say for middle-aged Swedish males, the mortality rate is roughly half that for United States middle-aged males. Similarly, on the variation in the amplitude change in such measures as unemployment in countries like Sweden, and in fact in other European countries—and certainly this is true of little European countries as well—their experience per se is nothing like that of the United States on the issue of cyclical variation and on the issue of the actual level of employment.

So we might be talking about quite different animals here. If we consider indeed two types of populations of drinkers—one, an affluent drinker, as in the phrase “drunk as a lord”—we may be talking about a population of individuals whose habits are related much more closely to the idea proposed by Ledermann of habitual drinking attached to a certain style of life, with a pervasive consumption pattern very easily traceable to social damage with a nearly linear equation. On the other hand, when we are talking about serious problem drinking, which might occur in fits and starts depending upon when the problems occur (however, it is rather strongly related to fluctuations in unemployment) at least in the United States, a character altogether in distribution-deterrent drinking, as associated with specific subpopulations, may be quite something else. So we might be able to learn something about the distribution impact of alcohol by comparison with certain European societies, which are relatively homogeneous in the character of their social class distributions and which may have

a lot to do with the relationship between consumption and ultimate social damage.

A second point has to do with the price issue. This is something I will deal with this afternoon, but because it was raised several times it appears to be important to the consumption story. The relationship between price and consumption, the best data we have that is over time, is extremely problematic. The idea that we ought to include income, as the Parker and Harman paper stated, is really very well taken here because real per capita income absolutely implies a decline in the increase of the price of anything, including the price of alcohol. So that controlling for per capita income would perhaps under the worst, or if you like the best, conditions mathematically dismiss the price issue altogether. Thus when most people have more money to spend, the price of all things diminishes, once one controls inflation, which is all the increase of price means. However, it is possible that price in itself may have affected its own, which would require extraordinary statistical experimental designs—in any case, a thoroughly unresolved issue. Yet from historical evidence thus far accumulated (studies of my own for example) in which one includes over time the income variable, any measure of the price of alcohol, the price of any other commodity or of all commodities, or the price of disposable income over time vanishes in the face of the income variable. Now here I return to the Parker and Harman relation on including it as a measure. The problem is, I'm afraid, that in their own paper they do not include it in their scheme; it is however a variable with extreme sensitivity and potency.

John Seely: The thing that can't be overlooked, I think, is that I intended the original work to be in large part a test of the seriousness of the Ontario—or any other Canadian—government. Therefore, it had to be a prototype that had never before been considered by the State. It had to answer to a number of their objections. One, that it was either impossible or was wrong to change attitudes, and that nothing could be done without changing attitudes. Two, that the Government couldn't afford the loss of profits to the distillers, because the distillers were their supporters. Three, that the Government couldn't afford the loss of taxes if consumption did go down. And so on.

We demonstrated that the curvilinear shape of the relation between consumption and price and between consumption and disease, permitted a simultaneous reduction in cirrhosis death rates *and* a sufficient increase in revenue under decreased consumption to give them a fund which they could, if they wished, distribute between themselves and the distillers. Thus, while they

were keeping the profits of the distillers the same, they could keep the revenue from liquor the same and have a substantial tax revenue (from liquor) left over to spend on education and other things they said they were unable to finance otherwise. This demonstration and challenge was then a very serious test of how they would deal with a response to the political objections they said attached to the various suggestions that had been put forward previously. And when they refused to consider the response seriously, we learned something. When we watched the cabinet jiggling this hot potato, we also made a political-economic discovery. One complaint was that we hadn't dealt with all "intervening variables." It was also part of their argument that, in effect, we should sit there and try and figure out all the intervening variables and all the fallout down to the last person who might be affected, so that it would be 10 or 20 years before they would act on the plain implications of the studies.

Here we had what seemed like a clear demonstration, in a vast number of jurisdictions at any one time, or in any jurisdiction over a very considerable period of time. Whatever else was proven, that relation was established as significant whether or not there were intervening variables and whether or not these involved heavy drinking.

The typical early studies for every State (and every Province but one) established a profound relation between price, taking income into account (that is, price as a fraction of disposable income), and the liver-cirrhosis death rate—to which innumerable doctors attested who had no interest in alcoholism and weren't acting in response to a stimulus they understood to be connected to alcohol. There was a correlation vastly over the point nine zeroes. This really *did* afford us a political test. And I can't help saying that the material that is not on display before us here is the more important: that is, the intervening arguments as to what the effect would be on government income, and the demonstration that reduced consumption would *not* destroy the distilleries or the breweries upon whose votes the Ontario government depended for election. This material would knock those two arguments out with one blow, so that a relatively clean field would be opened for argument. Among all your papers, that is what is at fault historically. In defense of my friends and colleagues at ARF—not that they need defense—I should have said that at the same time we were pursuing a host of other lines of inquiry, including cultural and attitude studies. But, to return to these price studies—we made very, very considerable refinements, taking income into account. Where we first put price over income, (P/I)

we later put price over income to the n^{th} (P/I)^r, leaving n to be estimated as a parameter, because we thought that it might vary between 1.0 and 1.5, as it does with reference to other social phenomena. It might be somewhat closer to 1.5. But that would have tightened the correlation rather than weakened it.

However, the studies were primarily meant to have an historical function. And from my point of view, whatever may be said about refinement today, the fact is that the measures implied as worthy of experiment never received a test anywhere. To me, this indicates—particularly given the rejoinders of the Government at the time and since—a great deal about the seriousness of Government prevention efforts or programs (a topic with which I'd like to deal tomorrow).

If you have increasing affluence, you do not expect price, perhaps, to be particularly effective in reducing consumption. What price *will* do is it will limit the income induced increase. In that sense, and that is not a guess, there is sophisticated analysis on elasticities that are able to single out these things.

Griffith Edwards: Wolfgang Schmidt has put forth three propositions. These propositions deserve to be considered very seriously; the smaller points, as Reg said, are not absolutely essential to this morning's debate. It is vital for the United States of America to know whether these propositions are or are not well founded. And there should be no shirking the issue. That would be a waste of all our time, which would be intellectually dishonest and a waste of the funding that put this meeting together. My own assessment is that Wolf's arguments win hands down. He has considered every point in considerable and objective detail, and his three essential premises stand. If that is not so, I would very much like Doug and Marsha to take their rejoinder further. And it may well be that I'm in the unusual position of someone watching a court scene, totally persuaded by the last attorney, and when I hear Attorney Doug speaking, I shall once more be persuaded in the opposite direction.

The critique provided on the Ledermann distribution data is important. But Wolf's suggestion is that the review was inadequate and incomplete; it missed very important evidence and misunderstood other evidence. We need to clarify this issue. I've heard Wolf and his colleagues make presentations in various parts of the world, including not far from here, and each time I've seen the debate become alternatively heated and tangential.

Marsha Harman: First I'd like to respond to a few things. Reg Smart raised the point of why we are quibbling over the number of angels on the head of a pin although he considered it a little more gently and nicely. I think we have to realize that, one,

we can all agree that the distribution-of-consumption theory involves a pragmatic set of proposals addressed to real problems; the model tries to use tangible indicators and to plan workable programs. However, we know the distribution on which the programs are based is important not only for academic reasons but for some very pragmatic reasons and is therefore in line with the same pragmatism the entire set of thought presents.

The distribution-of-consumption literature consistently tries to predict the size of the target population of heavy consumers of alcohol. This is very important when you present a proposal for policy; people are going to say, well, how many people are going to be affected? How many at a time? Which leads to the second position, the cost-setup analysis, which is something we have to live with today. People are going to want to know not only how many are going to be affected; they are going to want to know afterwards how many were affected. By having an accurate prediction, you have a better chance of being on target in presenting this information and therefore of showing cost-benefit effectiveness afterwards, getting refunding and continuation programs and so on. So I think this is important.

Then, turning to some of the points raised by Grif Edwards, I think that what most of you recognize is that we are not saying that they are wrong, in terms of the very basic propositions of distribution theory; we are saying that the specifics are wrong and need to be changed. In fact, we'll agree on certain of these things, such as the need to change the maximum consumption level that goes into the equation. Our attempt to form a model uses this theory; it does not discard it totally. We were so much in agreement that we originally had a "baby with the bathwater" statement in our own paper, which we were persuaded to delete before we knew it was going to appear in Wolf's paper.

The work of Skog, which is one of our primary sources, very strongly supports the notion that you need a two-parameter distribution in heterogeneous populations, but one-parameter distribution does not apply only in homogeneous populations, which is exactly what Harvey Brenner said concerning the European levels. In other words, you really need either two distributions, or subsume the one-parameter distribution in the special case of the two-parameter and stay with the two-parameter distribution as with the gamma distribution.

In particular, I want to raise a point. Skog is saying that Ledermann's theory of the distribution of alcohol consumption is a remarkable example of scientific intuition and so on. But about four lines down, and not included in the quote, is Skog's state-

ment, "This does not mean, of course, that we should accept the Ledermann theory as the final truth." And he goes on to argue for searching for the cases of invariants, for the cases of variants, and later in a second quote, speaks of the need to develop a two-parameter distribution, to explore it in order to search for a better distribution. He says, giving you the full quote, "Even though for most practical purposes a simple one-parametric distribution such as the Ledermann distribution is likely to have a fairly good approximation." Okay, now the part that was not included—"it is nevertheless desirable to develop a theory which takes the fact (of population heterogeneity) just mentioned into consideration." So we feel in a sense that that has been a little bit skimmed over and that there is justification for this changed distribution that would give you better prediction. But we also do have a lot of ground of agreement—not because we want to be a mutual admiration society but because the thinking of ARF has been moving in this direction. Our thinking when we started out was very much opposed to the distribution-of-consumption theory. Our initial assumption was that anything that's putting people into these kinds of holes in terms of roles of distribution must be bad. And on doing the review, we discovered that we could not sustain that position and had to modify it. So what we have really is change on our part, change on the ARF position, and we're slowly coming into agreement. And this, of course, should be happening if there's to be any kind of evolution of the way we think about problems and of working out a set of solutions.

Sherri Cavan: What has happened is that there's now been a shift from the notion of "problem drinker" to "heavy drinker." And it's not at all clear in my mind that the heavy drinker and the problem drinker are one and the same. Again, if you take for example the frequency of quantity rates, it's quite clear that people in upper socioeconomic status are heavier drinkers, but not necessarily problem drinkers—not the ones that are involved in things like child beating, nor the ones that are involved in things like public drunken and disorderly conduct. So that in fact the focus of the distribution theory becomes very different from the focus of the sociocultural theory.

The second point I'd like to make is a somewhat analogous kind of situation. In the last 5 or 6 years, the price of marijuana on the street has, for all practical purposes, tripled. It's gone from about \$10 an ounce to about \$30 an ounce. Nonetheless, survey research suggests that this has not, in fact, led to a decrease in the number of people who are smoking marijuana. In fact, more people are smoking marijuana than ever, and those who smoke are smoking

more. So this is a case where in fact the increase in price seems to have no effect in terms of usage.

The third point I'd like to make is that within the context of the general problematic concerns of public health, most of us, or probably all of us, are well aware of the relationship between cholesterol levels and heart disease, and the relationship between animal fats and eggs and the level of serum cholesterol; and I wonder whether anybody would seriously propose the idea of raising the price of beef and eggs as a way of controlling heart disease in America. I find this quite analogous.

Allan Williams: I would like to follow up on Reg Smart's comments, which started off the discussion. As I'm reading the papers on the distribution-of-consumption model, I have some problems with the theory and some questions about it, mostly minor. On the whole it seems plausible. If it's plausible, I think it boils down to an empirical question: Does it work? And the question I have, the question I'm interested in as a researcher, and I think the question that ought to be addressed here is, "Is it possible to have an adequate test of the theory?" That is, realistically, what are the possibilities, practical and otherwise, for instituting the changes in availability that will lower per capita consumption and in turn impact on heavy drinking and problems associated with heavy drinking? And can we evaluate these changes using scientifically acceptable techniques, not simply looking at before-and-after changes? This way we can have an adequate test of the theory. And if we can't do both of these, and I emphasize both, I don't think we'll ever make any progress in the prevention area.

Donald Cahalan: I wanted to raise a public policy point (not a research point) that has troubled me about a research group making specific policy recommendations for the governments to carry out. This is not disputing the essential plausibility and reasonableness of the three proposals. Because at least what they do is that they follow the Aesculapian dicta of "Above all things, do no harm;" or do a minimum amount of harm when you're making changes in legislation.

Now this is not raised in the sense of captious criticism; I'm really uncertain about the whole business. In my opinion, I think it's wiser to separate research in policy recommendations as a tactic in dealing with legislation. Because in that way you don't get the researchers in a position of advocacy of a particular policy and so on, which can sometimes contaminate other researchers and render suspect the objectivity of the group. Also, the politician, in his behavior, is going to run far ahead of the available evidence anyway. I'm not saying you shouldn't talk with them, but avoid making a specific recommendation.

Reginald Smart: Well, when does one get specific? One has to get specific sometime, and the choice clearly is when *is* the best time. You can't always talk about generalities. I mean one has to answer "What is the way to reduce alcohol-related problems in a given population?" Legislators often want to know that, or act as if they want to know it. Researchers are always going to say, "Well, we really don't know, we have to have more research." And if that's the case, then the distilling industries know it, the breweries know it, certainly the lobbyists know it.

Donald Cahalan: My point is that you should lay out the assumptions and the cost benefits of alternatives. And that's not just retreating behind a fog of figures and evasions, mind you, but letting legislators pick and choose from alternatives.

Wolfgang Schmidt: Everybody, I believe, would have to agree that a cost-benefit analysis of alcohol use would be politically very useful. Legislators, of course, love that approach to a problem. And in fact our government has asked us specifically to do such an analysis. However, we feel that we do not have the data base that would be required for such calculations. It was interesting for us to hear that alcohol researchers in Finland were given a similar task by their government. As a response they produced a very extensive document indicating to their government that the cost-benefit statement they requested was not feasible.

So while everybody would agree with you, it would be my response that it would be a marvelous basis for policy decision. At the same time, we have to keep in mind that our research is meant to improve the basis for policy decision; if it fails to do that, it is practically a useless enterprise. And we feel that, assessing the totality of evidence, very few modest statements are possible. We feel that we owe it to the taxpayer who pays our salaries to make this information available.

John Seeley: I would be inclined to seize the other horn of the dilemma and throw the question back to the inquirer. I would start from the position that research is the pursuit of politics by other means. There's no question about it. The 30-year attempt to delude ourselves and the public that it's otherwise (somehow value-free, or having a point of departure in some angelic realm) is unproductive and self-deceptive, and disguises from the public the nature and utility of research.

Given that point of view, the question you ask, I think, is still a legitimate question. That is, how, why, and when should you apply such strategies of deceit that your politics are not evident to the person you mean to affect, completely or otherwise. So whether or not you present a cost-benefit analysis, choose this point of comparison or that one—in part, I agree with Ms. Cavan—

is itself a politico-strategic question. (It's not that the idea of taxing beef may seem crazy—the idea of taxing horsepower doesn't seem crazy, and it does seem to have led to some difference in the shape and structure of the transportation industry.)

I think that the strategic or tactical question you ask really depends on the inner and outer strength of the research community. But, it's a question the research community can't answer until the community realizes what is the nature of the research. And this will not happen until those who have substantially different political and moral positions sort themselves out, agree to work together, find those colleagues who agree as to the interests they want to serve, and find a way to stand behind what they believe in. The whole research enterprise will then become something entirely different from what it's now pretending to be. But within the context of suppositions that research is what we've pretended it is (for 30 or 50 years or since Weber), one can do little that's different or better. I say that's a question of the presentation of the researcher himself, not in everyday life, but in professional life.

Sherri Cavan: But the thing is, for example, within the context of this conference, we're not just here to discuss our theoretical insights into the problem of drinking. It has been quite clear that the thrust of this entire discussion has to do with certain kinds of control and with changing certain patterns in human populations. It's not just idle talk that happens to be about alcohol.

Harvey Brenner: Most historic propositions of the past that have a specific policy have been put forward. The distribution model is quite conceivably dealing with two entirely different modes of prevention, one having to do with total interdiction of use, or prohibition, and the other with price mechanism, for example.

I find price mechanism very difficult to deal with as an issue. It doesn't work very well in the United States. It depends on what variables one puts into the equation. That's the key to all these complex multivariate analyses. But, getting back to Sherri Cavan's point. She raised the marijuana issue, but something a lot more ubiquitous than marijuana and far more damaging—something that has varied and increased in price enormously, has not led to any reduction in use—that is cigarettes, probably the single most damaging ingestive control-pattern consumable that is on the market.

Wolfgang Schmidt: That is not so. Cigarettes are highly price-elastic. There are some very good studies that consider elasticity. As a matter of fact, a great deal of the variation in cigarette con-

sumption through time and geographically is very, very, explainable on the basis of the price.

Robin Room: I see two major issues arising out of Wolf's three points. One of them is essentially a political issue—to what extent do your politics and the politics of the polity allow for the notion of governmental intervention in saving people from themselves? One test case is the issue of cirrhosis, which certainly affects those around the person but otherwise has no strong effect on the economy at large. It may lose the production of that person, but in an economy that has less than full employment that may be a benefit to the economy rather than a cost.

So that there is a fundamental political issue, which I think underlies a good deal of the discussion, that is, "What are our personal politics, and what are the politics of the particular polity we're talking about concerning saving people from themselves by governmental action, and concerning what kind of governmental action we're willing to contemplate?" It is very clear that there is a lot of differentiation in our mind between a government policy of compulsory sterilization, for instance, and a government policy affecting the price structure. There is a considerable range of possible government policies and, depending on the policy, our ethics and politics might be rather different.

The second issue is the issue of the relationship between the fact that cirrhosis may, on the one hand, be used as an indicator, but on the other hand is only one of the drinking-related problems possible for the society and for the individual. Some of the other problems seem to behave rather differently—some seem to behave rather like cirrhosis. But when it comes to social problems that are defined partly by the societal reaction to the drinking, then we are in a rather different circumstance in which any measures that tend to focus public attention on heavy drinking probably also tend to increase the amount of dissension within the society over alcohol issues, and so by definition raise the rate of social reaction to drinking problems.

Griffith Edwards: There are few examples of social science findings being taken through to social policy, unless the researchers take an active hand in furthering that connection. That's not so if you discover penicillin. If you discover penicillin, people can immediately see that it works. Penicillin meets little social resistance, but the questions we're dealing with now will meet considerable social resistance, and we are dealing with issues about which it is difficult to give a firm, final, and honest statement. We are not like the penicillin discoverers; we will always be one-quarter crossing out what we've just said, and showing the fine variety of our own doubts.

An issue that needs tidying up is the doubt in many people's minds as to whether heavy drinkers, the alcoholics, people suffering from a disease will be affected by increasing the price of drink. The fear is that, far from being so affected, they will rob the baby's piggy bank, let their spouse go barefoot, and continue to feed their habits. Whereas if I were having half a bottle of sherry a week and the price went up, I would cut down on my drinking, it might be argued that the alcoholic is hell-bent and can't alter his behavior. This is where medicine has done a disservice to societal understanding. It has offered a picture of the disease of alcoholism as something absolutely monolithic, like nothing else known to man or woman.

No physician of any experience who cares to listen to his patients will ever see people behaving in that machine-like way. He will be aware that the alcohol-dependence syndrome is one variety of dependence on a drug. It is a mystification of alcohol dependence to picture it as something totally special and different from any other form of drug dependence. The drug addict has a wide margin of choice as to the level at which he may adjust his dosage.

Anyone who knew the heroin user in the very lax prescribing system we used to have in England will have come across patients who were taking 12 grains (720 milligrams) of heroin a day—a luxurious dose. Under different supply conditions, these patients, while still addicted, could operate on a much lower dose. The dependent alcoholic, you will find, has a choice of perhaps two to three bottles of spirits a day, ranging downwards when money is short to say three-fourths of a bottle a day—all according to whether he just got his income tax rebate, whether he is working at a pub where he can filch the stuff, whether his grandmother has left him a legacy, whether he's on a holiday in France, whether he's getting depressed, or whether he's trying to cut down. Now, that person is also affected by the price of liquor. He may not have the same degree of responsiveness as the rest of us, yet he has responsiveness.

Another way medicine has accidentally done a disservice is to picture the really serious problems as neatly segmented by a unitary syndrome of alcoholism. Not everyone who is suffering a serious cluster of problems, going in and out of serious clusterings, or cirrhosing his liver is alcohol dependent.

Physicians have been telling us that there are people who just can't stop drinking and must go on, who will wreck their lives rather than stop drinking. I don't think that should be used as a valid scientific objection to what Wolf was saying; the physi-

cians' evidence is anecdotal. All that can be provided at present is contrary anecdotal evidence saying that the physicians, to an extent, misled you. They haven't listened to their patients.

Sherri Cavan: The whole gist of the distribution theory deals with a mass, or large group of people, and only assumes that those target populations will in fact be affected by those price changes. We do not know. We know that the mean consumption production described in this discussion is an unacceptable solution because it smacks of prohibition, aspects of capitalistic production, and so forth. It seems to me that the notion of increasing the price as a way of controlling intake is a political decision that addresses the fact that some kinds of policy changes cannot be made and that if we accept these theories, then this is the best kind of policy change that can be made. It has a phenomenal class bias in it. It is quite clear that it would have a longer effect on people at the bottom socioeconomic status level and have much less of an effect on people at the top, whereas control of production would make alcohol less accessible in general, quite independent of price, and would have more chance to affect the overall consumption package.

Wolfgang Schmidt: One additional point should be made in the price discussion. The retail price of alcoholic beverages consists largely of taxes. Hence, the cost of alcohol is not very sensitive to forces in the marketplace. In fact, in Canada up to 80 percent of the cost of whiskey is made up of taxes. This is also the case in many parts of the United States. The decline in the *real* cost of alcohol that has occurred in many North American jurisdictions is largely due to the failure of governments to adjust the tax level deliberately in order to prevent decreases in the real cost during periods of inflation and thereby keep the price of alcohol in line with the cost of other goods.

Sherri Cavan: There was a time, for example, in England when the whole development of the gin industry was closely related to industrialization, when the prices were in fact considerably cheaper than they are now. So if you take a longer historical perspective, you have much more variation than you do if you just take the last 20 or 30 years.

Wolfgang Schmidt: In this connection, I believe that one has to ask two questions concerning price. One is, do we want alcohol to become less and less expensive? Because this is what has been happening over the last 20 years. Taking all the evidence into account, we would prefer stabilization of the price structure. Quite aside from that, the other question is, to what extent would one want to use price changes to produce a reduction in consump-

tion? But this is a very different issue because as I have pointed out earlier, the recent decline in the real price of alcohol is largely a result of a failure of government to adjust taxes to counteract the effect of inflation. Thus the real trend in the cost of beverage alcohol is not the result of conscious planning on the part of policy makers but is more likely due to an oversight.

Douglas Parker: I have a very brief comment. I feel very uncertain about what the implications are of introducing a change such as has been proposed by the distribution-of-consumption model. It seems to me that many questions remain unanswered in the predictions that can be made with the Ledermann formula, so that all kinds of overestimates and underestimates result. It is a real question what will happen to drinking patterns—what will happen to the relationship between price and income. Until those issues are resolved, I guess I am not very comfortable.

Marsha Harman: I find that same discomfort in the policy-making recommendations. From evaluating the data, I strongly feel that at least in the case of the United States where there is such a heterogeneous population—and I suspect that the Canadian population outside of Quebec is not that homogeneous either, Canada being a nation largely composed of European migrations—we really need to answer some basic questions before jumping into policy.

Availability and the Prevention of Alcohol-Related Problems

Reginald G. Smart

If there were no heads there could be no headaches, and if there were no alcohol there could be no alcohol problems. Obviously, availability is of exclusive importance as a necessary condition for alcohol problems. A more interesting question is whether and under what circumstances it is a *sufficient* condition. Of course, we have little hope of eliminating alcohol completely, even given the motivation. What we usually face in considering alcohol availability is examining the confusing effects of rather small changes in its extent. Apart from shortages created by wars or total prohibition, we are typically examining small variations in availability, such as outlets, prices, and the like. This is not to minimize the importance of small changes but to deplore the lack of opportunities to study large ones.

Another problem is that there are many ways in which availability can be defined. If the distribution theory as proposed by Popham et al. (1976) is accepted as the basis for preventive strategies, then any and all changes in availability are potentially important. Any change that could increase per capita consumption could conceivably contribute to alcohol problems. The purposes of this paper are to examine: first, the various ways in which the concept could be understood; second, problems in studying availability; and finally, results of empirical studies.

The Concept of Availability

There are various ways in which "availability" of alcohol could be understood. In the alcohol literature, it has typically been taken to refer to all physical (legal) arrangements made by governments to allow the purchase and consumption of alcoholic beverages. Usually, the concept of "alcohol-control policies" subsumes everything that "availability of alcoholic beverages" implies. This

has led to research into the effects of drinking-age changes, of the introduction of public drinking places, of prohibition, and of liquor store strikes. The concept of availability employed here is that concerned with physical availability—the specific alcohol-control measures employed except for price. A related concept would be "economic" availability, that is, the affordability of alcoholic beverages to the consumer. Mostly, economic studies examine official statistics on price and consumption. Seldom do researchers examine the impact of home production, which in some situations can mean that beer and wine are available for only 10 percent of the cost charged by commercial establishments. Studies of economic availability are not reviewed here because they require a different level of analysis than those of specific laws.

Relatively little attention has been paid to two other concepts that might be termed subjective availability and social availability. Subjective availability could refer to subjective or individual differences in how accessible people feel alcohol is to them. One's subjective availability is, of course, some function of the amount of energy or resources one is willing to expend to possess or consume something. Numerous researchers (Cohen 1964) have demonstrated that people's actions are often governed more by subjective estimates of cost and risk than by real costs and risks. Perhaps many of the smaller changes in alcohol availability have no effect on consumption because they have no influence on overall subjective availability. That is, the change made did not affect the amount of work or sacrifice necessary to obtain alcoholic beverages.

The concept of social availability—availability within small social or family groups—is also worth considering. We could study, for example, how one's friends' use of alcohol affects one's own consumption. It has been known for a long time that drinkers tend to have friends who are also drinkers (Straus and Bacon 1953). People may choose friends partly because of their similar drinking styles, but does availability have any influence on amounts drunk in social groups? Several proponents of the distribution theory have claimed that there is a *boule de neige* effect: if we are offered alcohol by friends, we feel obligated to reciprocate, and they to follow in turn. However, there appears to be no empirical study of this claim.

We could also be interested in how availability within a given household affects drinking. Presumably, if there is no alcohol on hand and stores are closed, there will be no drinking at home. But what effect do situations of high availability have? There are

occasions where large amounts of alcohol might be present, such as after a party and during Christmas or other gift-giving celebrations. Do these situations result in more frequent drinking or drunkenness for the social group affected by them? Is there more drinking in situations where there are large amounts of free drinks? That often seems to be the case, but again empirical evidence is scarce.

The concept of social availability could be extended to include family availability. We know from many studies that adolescents of drinking parents tend to be drinkers themselves more often than do children of nondrinkers (Smart 1976). Does availability have some unique effect that is exclusive of modeling or social influences? We might wonder whether just having alcohol on hand increases the likelihood that family members will start to drink at earlier ages and in larger amounts than do members of low availability families. This question has apparently not been researched.

Problems in Studying Availability

As mentioned earlier, the majority of empirical studies directly related to availability and alcohol consumption have been concerned with changes in alcohol-control policies or procedures. That is, studies relating social, subjective, and family availability have been rare. This has simplified the researcher's task in unravelling the various effects, but it has not made it easy. Even in studying the effects of changes in alcohol-control policies, numerous problems exist, a few of which are worth considering.

A major problem is that usually availability does not change very dramatically. The trend in most countries over the last 20 years has been toward a gradual liberalization in alcohol-control laws. However, few single changes would be expected to have a detectable effect on alcohol consumption or alcohol problems. As an example, here is a partial list of changes in alcohol-control laws in Ontario during the last 20 years. Similar changes occurred in other Canadian provinces during the same period. Major changes relevant to availability in Ontario in those years were as follows:

1. 1951. Votes of a local-option nature could be used before a license was issued, rather than afterward only.
Liquor could be consumed in a trailer or tent for the first time.
2. 1953. Food could be sold in taverns.

Persons applying for licenses had to advertise the fact beforehand.

3. 1956. Local-option votes were allowed in areas with populations of 50,000 or over.
1957. Liquor permit books were discontinued in favor of permit cards.
1960. Public inebriates could be treated after they were defined as recidivists.
Allowance was made for carrying liquor bottles from the store to a residence.
Liquor could be given as a gift.
4. 1962. Liquor permits were discontinued altogether.
Private clubs could have licenses in otherwise dry areas.
Hours of sale for on-premise consumption were extended up to 12 hours a day.
Liquor could be served through room service in hotels.
Motels and summer resorts were allowed to apply for licenses for the first time.
5. 1965. Wine could be made from all fruits as well as grapes.
No cancellation of licenses was allowed without a hearing.
The right to appeal interdiction proceedings was introduced.
Fines for breaking regulations were increased.
Hotels in small towns were allowed to apply for licenses.
The sale of liquor on airplanes was permitted.
The sale of liquor in theatres was permitted.
Public houses could be open between 6:30 and 8:00 p.m.
Music and singing were allowed in licensed dining premises.
Open air drinking on patios, in backyards, etc., was approved.

Non-residents could drink in hotel rooms.

6. 1970. Unsegregated seating of males and females in licensed establishments was allowed.

Licenses could be granted to resort centres in dry areas of the province.

7. 1971. It was no longer necessary to order a meal in order to have a drink in a dining lounge.

Drinking was allowed with meals on Sunday in licensed restaurants.

Alcoholic candies could be sold (up to 5% alcohol).

Permits were relaxed for special events such as fairs, festivals, winter carnivals, Oktoberfests, etc.

40 oz. bottles of spirits could be sold (26.02 was largest sold before).

Package stores were allowed to stay open until midnight.

Duty-free stores at airports were allowed to sell alcoholic beverages.

Drinking establishments were allowed to open after polls had closed on voting days.

Licenses could be issued to new premises without a waiting period.

Provisions were made for taking public inebriates to detoxification centres.

The legal drinking age was lowered from 21 years to 18.

8. 1973. Definitions of recreational facilities and theatres eligible for licenses were broadened.

Licenses were allowed for canteens on campuses of colleges and universities.

Provision of drinking-age cards was made (although not required for purchase).

9. 1975. Minors could be served alcohol by parents in their own houses.

Opened bottles could be transported to any destination.

During the last 20 years, per capita consumption in Canada increased from 4.9 liters to 6.4 liters of absolute alcohol. What proportion could be attributed to increased availability as a whole or to any of the individual changes noted above? We cannot answer this question easily because, as is often the case, several changes were made at the same time; and these changes did not appear in isolation—there were large increases in affluence, the sobering influence of the temperance movement declined, and advertising of alcoholic beverages increased as did people's desires to drink (Schmidt 1977). Methods for isolating the influence of each of these factors do not seem to be at hand. Politicians who create the changes usually argue that they are merely keeping up with public opinion, i.e., that the desire to drink more precedes rather than follows increases in availability. It is difficult to dispute this proposition at this time, however unacceptable it may seem for those who agree with the distribution approach to the prevention of alcohol problems.

Empirical Studies of Changes in Laws Governing Availability of Alcoholic Beverages

Despite the problems mentioned above, numerous studies have been made of laws pertaining to availability. Few of these are concerned with overall measures and large changes; the majority concern rather specific changes. The large changes chiefly involve the effects of prohibition and wartime restrictions that resulted in extremely low availability. The smaller changes are related to age of purchase, number and type of outlets, and hours of sale.

1. Overall Studies of Availability

Bruun and associates (1975, p. 83), in an important review of the field, concluded that "the availability of alcohol is an important factor in the general level of consumption." However, studies examining the total range of variables that may affect availability are difficult to find. Bacon (1971) studied nine communities in five States and concluded that variations in availability had little effect on consumption. There appear to be no studies that have examined general availability in a wide variety of jurisdictions for effects on consumption. Recently the author examined the relationship between overall availability, per capita consumption, and alcoholism rates in the 50 States and the District of Colum-

bia. The availability of alcoholic beverages was assessed by giving each State a score on eight items concerning legal beverage control. These scores have been developed by Matlins (1975) for a study of alcohol-control laws that was financed by NIAAA. Efforts were made to partial out the effects of availability on consumption from those related to income and urban-rural differentials in the various States, because they are known to be related to per capita consumption.

The availability scores were developed to take account of all legal and control factors that might affect the availability of alcoholic beverages. Initially a 22-item list was developed. Eight items in this list were selected as more important than the others in influencing availability. The selection was made using expert judgments and a Delphi panel approach. The final eight items comprised minimal legal age for purchase, limitations on availability for off-premise sales, limitations on availability for on-premise sales, density of outlets for on-premise purchase, Sunday retail sales on-premise, weekday closing hours for on-premise sales, Sunday retail off-premise sales, and weekday closing hours for off-premise sales. Each State was defined for each item as falling in the high, medium, or low category. Values of 6, 4, and 2 respectively were given to each category. Total scores could vary between 16 and 48, with the latter indicating the greatest availability. No effort was made to differentially weight any of the eight factors.

The per capita consumption data and alcoholism data were taken from Efron et al. (1974). Data on average income and percent of urban population in each State were taken from the *Statistical Abstract of the United States* (1973). Table 1 shows all of the data by State.

Partial-correlation analyses were made in which correlations were calculated between availability scores for States and both per capita consumption and alcoholism rates, alone and with the effects of average income and degree of urbanism controlled or partialled out.

The correlation between per capita consumption and availability was positive and significant ($r = .41, < .01$). However, the correlation between per capita consumption and income is even greater ($r = .60$), and that with urbanism was also positive. The partial correlations show that much of the correlation between per capita consumption and availability is accounted for by income and urbanism. Tests of significance for the partial correlations were made with the T tests described by McNemar (1962). When income is controlled the correlation is nonsignificant;

Table 1.—Availability scores, per capita consumption, income, urbanism, and alcoholism rates in the 51 jurisdictions

State	Alco-hol Avail-ability Score	Per Capita Consump-tion (U.S. Gal.	Per Capita Personal Income (Dollars)	Percent of Popu-lation in Urban Areas	Rates of Alcoholism
		Absolute Alcohol Drinking Population)			(per 100,000 adults, 20 and over)
	1974	1972	1972	1970	1970
Alaska	44	3.86	5162	48.4	3110
Wyoming	42	2.85	4345	60.5	2300
Nevada	40	6.19	5215	80.9	6770
Arizona	38	3.04	4300	79.6	3110
Hawaii	38	2.48	4995	83.1	1780
Louisiana	38	2.46	3528	66.1	3980
Nebraska	38	2.50	4341	61.5	3087
New Jersey	38	2.91	5126	88.9	4930
New Mexico	38	2.81	3656	69.8	3160
Rhode Island	38	3.00	4399	87.1	6070
California	36	3.24	5002	90.9	6610
Delaware	36	3.17	4983	72.2	4040
Michigan	36	2.77	4817	73.8	4460
New Hampshire	36	5.42	4092	56.4	3660
New York	36	2.86	5319	85.6	5500
West Virginia	36	1.75	3574	39.0	3130
Colorado	34	2.98	4449	78.5	4240
Illinois	34	2.91	5126	83.0	5140
Maryland	34	3.04	4897	76.6	4100
Massachusetts	34	2.93	4870	84.6	5850
Minnesota	34	2.50	4332	66.4	3400
South Dakota	34	2.16	3716	44.6	2640
Wisconsin	34	3.37	4207	65.9	5110
District of Columbia	32	6.54	6383	100.0	5430
Indiana	32	1.89	4391	64.9	3940
Iowa	32	1.86	4318	57.2	2800
Maine	32	2.60	3571	50.8	4290
Missouri	32	2.28	4206	70.1	5090
Montana	32	2.81	3897	53.4	3730
Tennessee	32	1.74	3640	58.8	2920
Texas	32	2.41	4045	79.7	2830
Vermont	32	3.92	3865	32.2	4170
Connecticut	30	2.73	5342	77.4	4940
Kentucky	30	1.94	3601	52.3	3290
North Dakota	30	2.69	3718	44.3	3570
Oregon	30	2.67	4296	67.1	2610
Alabama	28	1.65	3333	58.4	1830
Arkansas	28	1.52	3357	50.0	2890

Table 1.—Availability scores, per capita consumption, income, urbanism, and alcoholism rates in the 51 jurisdictions
(Cont'd.)

State	Alco- hol Avail- ability Score	Per Capita Consump- tion (U.S. Gal. Absolute Alcohol Drinking Population)		Per Capita Personal Income (Dollars)	Percent of Popu- lation in Urban Areas	Rates of Alcoholism (per 100,000 adults, 20 and over)
		1974	1972			
Georgia	28	2.25	3846	60.3	2550	
Idaho	28	2.43	3635	54.1	1990	
Ohio	28	2.71	4512	75.3	4340	
Pennsylvania	28	2.32	4447	71.5	4360	
Virginia	28	2.23	4258	63.1	2370	
Florida	26	3.23	4188	80.5	3590	
Mississippi	26	1.88	3063	44.5	2320	
Utah	26	1.56	3745	80.4	2070	
Washington	26	2.71	4476	72.6	2910	
Kansas	18	1.72	4593	66.1	2490	
Oklahoma	18	1.80	3802	68.0	2140	
North Carolina	16	2.44	3448	47.6	2760	
South Carolina	16	1.93	3721	45.0	2050	

however, with urbanism controlled availability and per capita consumption are still correlated ($r = .362, p < .01$). When the correlation is controlled for both urbanism and income, then availability and consumption are unrelated.

Similar results are obtained for alcoholism rates that are also positively related to availability, but more strongly related to income and urbanism. When the effects of income, and income and urbanism together, are controlled, availability scores and alcoholism rates are not significantly correlated.

Overall availability of alcohol as indicated by control laws may have at best a spurious correlation with both per capita consumption and alcoholism rates. The positive correlation disappears when the effects of average income and degree of urbanism are partialled out. This suggests that slight changes in availability may have relatively little effect upon either consumption or alcohol problems, provided neither income nor urbanism is changed.

A number of considerations about these data are important. The availability scores are constructed to give each element, e.g., opening hours and drinking age, the same score, and this is an arbitrary rather than a realistic weighting. Unfortunately, price has not been included as an item in the scores as all the elements relate to

legal controls. Also, cross-national comparisons involving a variety of countries with *widely* differing availabilities and levels of per capita consumption might give different results. It is unfortunate that cross-national comparisons are so difficult in this area. There seems to be no source for assessing availability for various countries. Also, it is difficult to study how availability changed over time in the various States because there are no sources of information regarding State laws at given points in time.

2. Effects of Reducing Legal Drinking Ages on Drinking and Drinking Problems

Recently concern has been expressed regarding the effects of reductions in the legal drinking age that have occurred in many jurisdictions across North America; some of these jurisdictions (e.g., Saskatchewan and Minnesota) have even returned to former legal age limits. Table 2 shows the provinces in Canada and States in the United States that have reduced the legal drinking age. All 10 Canadian provinces and 26 of the States have implemented age reductions, often as part of a movement to create a single age of majority, usually 18, at which most legal rights are achieved, e.g.,

Table 2.—Recent changes in drinking age by Province (Canada) and State (U.S.A.)

<u>From 21 to 18</u>		
Connecticut (1972)	Michigan (1972)	Vermont (1971)
Florida (1973)	Minnesota (1973)	West Virginia (1972)
Georgia (1972)	New Hampshire (1973)	Wisconsin (1972)
Iowa (1973)	New Jersey (1973)	Alberta (1971)
Louisiana (1973)	Rhode Island (1972)	Manitoba (1970)
Maryland (1975)	Tennessee (1971)	Ontario (1971)
Massachusetts (1973)	Texas (1973)	Prince Edward Island (1972)
<u>From 21 to 19</u>		
Alaska (1970)	New Foundland (1972)	British Columbia (1970)
Arizona (1972)	Nova Scotia (1971)	North West Territories (1970)
Idaho (1972)	New Brunswick (1972)	Yukon (1970)
Wyoming (1973)		
<u>From 21 to 20</u>		<u>From 20 to 19</u>
Delaware (1972)		Nebraska (1972)
<u>From 20 to 18</u>		<u>From 19 to 18</u>
Hawaii (1972)		Montana (1973)
Maine (1972)		Saskatchewan (1972)
Quebec (1971)		

the right to vote, to obtain credit, and to sign contracts. At the time of the change there was little realization that such change might create drinking problems among young people.

The trend toward reductions in the legal drinking age began in 1970; since then, a number of preliminary studies have been undertaken to examine some of the consequences of the changes. Many of these studies have been conducted without adequate comparison or control groups against which the effects of legal changes could be tested. Of necessity, some studies were conducted with haste due to lack of warning about forthcoming changes in the law (e.g., in Ontario); several studies have been obliged to rely on post hoc analyses with or without the advantages of data extending over a long time period prior to and following changes in the law. Several empirical studies are unpublished. Similarly, no critical review of research in this area has been published; however, one is in press (Smart and Goodstadt 1977).

Expectations as to what consequences might ensue from age changes vary with the view generally taken about alcohol-control policies. For example, Wilkinson (1970) argues a type of "forbidden fruit" theory in which alcohol is seen as especially attractive to young people because of its illegality. According to this view, it is expected that once it becomes legal for young people to drink, drinking norms will develop. Wilkinson recommended lowering the drinking age to 18 in all jurisdictions. On the other hand, those in favour of the distribution theory (e.g., Popham et al. 1976) argue that liberalization will contribute to higher per capita consumption and hence to alcohol-related problems.

One could have argued that changing the law would have no effect on consumption at all. It is known that many young people drank before the law was changed; new laws may thus only legalize the status quo. It has also been observed that many European countries, such as Britain, have always had low drinking ages with relatively low per capita consumption and alcoholism rates. However, France and Italy also have low drinking ages and lead the world in both alcohol consumption and its problems. International comparisons may be of little real value in relation to before-and-after studies in single jurisdictions.

Few studies have been made of the effects of the new age laws on alcohol consumption. Only Canadian studies have been identified, and of these all but one refer only to Ontario. Most of these studies have no control or comparison groups comprising, for instance, a jurisdiction with no legal change that would provide a basis of comparison for evaluating the effects of these changes.

However, studies of the effects of age changes on driving have been made in a variety of areas.

In Ontario, the law was changed in July 1971 to allow 18 year olds to drink where formerly the drinking age was 21. Smart and Schmidt (1975) reported a study of Toronto high school students that had been conducted every two years since 1968. The study was cross-sectional and employed the same sampling system in all years but did not re-survey the same students. The sample included approximately 20 percent of available high school districts; 120 students of each sex were selected from each of grades 7, 9, 11, and 13. In 1970, some 6,882 students were surveyed; in 1972, 6,627 students participated. Between 1970, 1972, and 1974 the proportion of drinkers increased from 60 percent to 70 percent to 73 percent; all these changes were significantly different. It is, however, interesting to observe that the 1970-1972 increase was greater than that occurring between 1972-1974. (A change in the question between the 1968 and 1970 surveys precludes easy comparisons for these years.)

In May 1972 high school students were also asked directly about changes in their drinking since the new law went into effect: 27 percent reported no drinking at all; 41 percent said there had been no change; 20 percent reported more drinking, 4 percent less drinking; and a percent said they started drinking.

Another Ontario study reported by Smart and Schmidt (1975) was conducted in February 1972 and inquired about drinking among college students 7 months after the new law. First-year students at community colleges and universities ($n = 448$) were asked whether the new law made a difference in how often they drank. Most (57 percent) reported no change. However, 25 percent of males and 21 percent of females reported an increase in the frequency of drinking. The increases were greater among students who were younger and among those who were more frequent drinkers. However, very few students claimed that they drank more on each drinking occasion. The type of drinking occasion also changed after the new law. About 54 percent of students reported increased visits to bars and pubs, but only 19 percent reported more drinking with parents. About 40 percent reported more frequent purchases at package stores after the new law. The percentage of students who went to bars or pubs three or more times a week doubled from 3 percent to 6 percent. Again, the largest changes were among younger students, those most affected by the new law. For example, 81 percent of those 18 years of age, but only 45 percent of those aged 21, reported never having made purchases before the new law. The major effects of

the new law were not on family drinking but on drinking in bars and pubs and on purchases from package stores.

A study by Schmidt (1972) examined the effect of the Ontario law on sales of alcoholic beverages. Separate analyses were made for on-premise sales. The new law increased the population of those legally permitted to drink by only 8.1 percent (those between 18 and 21 years of age), many of whom were drinking before the new law. Schmidt calculated an expectancy for the proportion of total annual sales accounted for by sales between January and July and by the period August to December, taking into account the increase in population after the change in the drinking age law. He found that the increase in on-premise consumption was greater than expected but that in off-premise consumption was lower than expected; this increase occurred for beer, wine, and spirits.

Another study (Smart and Finley 1975) examined the effect on per capita beer consumption in each of the 10 Canadian Provinces, together with a control Province with no change. It was found that—

1. Considerable inconsistency occurred in apparent beer consumption in the year after the change—no striking change took place;
2. There was an overall increase in beer consumption in the pre-post comparison, but this increase (overall) was not greater than in the control Province; difficulties in finding a control Province not affected by earlier age changes made firm conclusions difficult; and
3. In five Provinces there was a decrease in consumption.

Both Hammond (1973) and Smart and Schmidt (1975) report reactions of school officials to the changes in the legal drinking age 6 months after the change. Hammond's study involved 354 principals, "which was only 46 percent of the total"; it is not clear whether the remainder did not reply or were not surveyed. However, 44 percent reported that school functions such as dances were more of a problem. Twenty-six percent reported increased drinking at noon hour and 32 percent reported more problems with drinking during school hours. Most principals (66 percent) thought there was more drinking among 15- to 17-year-olds.

Smart and Schmidt (1975) reported the results of a survey of 220 vice-principals conducted in the Toronto area 7 months after the change in the age law; 86 percent replied. Slightly more vice-principals were against the new law than were for it. Those who were against it reported more drinking problems in their schools. It is uncertain whether their unfavourable attitude to the law led

to or followed their negative perceptions about the effects of the law. About 28 percent reported no increase in drinking, but two-thirds said there had been an increase; the remainder did not reply. Overall 40 percent reported more drinking at noon; 34 percent, more alcohol-related disciplinary problems; 20 percent, more drinking-related absenteeism; and 23 percent, more student hangovers. Studies of this type are of interest but suffer from the confounding effects of attitudes, distorted memory, and lack of adequate baselines for the observed changes. Pre- and post-studies using actual school records have not yet been conducted.

Smart and Finley (1975) studied increases in youthful admissions to alcoholism treatment facilities before and after the change in Ontario. Prior to 1971 there were very few admissions to alcoholism treatment for persons 21 or under. However, by 1974 this age group accounted for 4.4 percent of all admissions. The first significant change in the number of admissions during the years 1964-1974 occurred in 1971, the year of the Ontario age change. Of course, these increased admissions could reflect (1) a change in acceptability of treatment among youthful alcohol abusers, (2) a decreased delay between first appearance of a problem and seeking treatment, or (3) an increased incidence of alcohol problems among young people. More research will be necessary to determine whether the age change was the most important factor in these increased admissions.

A variety of studies in several jurisdictions have shown the effects of age laws on accident involvement. Several of these studies have employed comparison jurisdictions experiencing no legal change; there is however, little agreement at the present time concerning the effects of the changes:

1. There is some dispute as to whether the entire increase in alcohol-related accidents is due to increased availability as opposed to other factors, such as increased police surveillance of young people (Zylman 1974).
2. Alcohol-related automobile accidents usually show post-change increases greater than in comparison areas where no legal change occurred. Some studies have used data (e.g., data on night-time fatal accidents) unlikely to be affected by increased police activity. These increases do not occur in all States (e.g., Vermont) but the reasons for the inconsistency are uncertain (Schmidt and Kornaczewski 1972; Whitehead et al. 1975; Williams et al. 1974; Douglass and Filkins 1974).
3. Changes in the drinking law have probably affected the accident experience of those aged 15-17 as well as those aged 18-20 (Williams et al. 1974).

Effect of Number and Type of Outlets and Hours of Sale on Consumption

All developed societies have some controls on the number and type of outlets and the hours of sale for alcoholic beverages. The variation in extent of control is considerable, and changes in the controls create opportunities to examine the effects of various sets of controls. It is worthwhile to examine not only the correlational studies but also several sets of situations in which controls are changed. In one, rather small changes in a single control measure are made and in the other, much larger ones, e.g., prohibition by public policy or wartime shortages. Small changes would include studies of the sale of liquor by the drink, introduction of Sunday drinking, lengthening of hours open, etc.

Some studies (Mass Observation 1943; Popham et al. 1976) have found that correlations between drunkenness rates and outlet rates (per population base) are negative, i.e., that public tolerance is apparently similar for drunkenness and outlets. Also, correlations between rates of outlets and per capita sales are insignificant (Popham et al. 1976).

Small Changes in Availability

Several small changes, i.e., changes in a single control measure, have had little effect. For example, Bryant (1954) found no effects on consumption from the introduction of liquor by the drink. Dewar and Sommer (1962) found no effects from replacing a male beer parlor with a tavern serving both beer and wine to both sexes. Smart and Docherty (1976) failed to find the expected effects of the introduction of on-premise consumption on drinking-driving problems. In fact, the control area had fewer drinking accidents after on-premise consumption was introduced to the experimental area. Perhaps under the old system people had to drive further for a drink, so were more exposed to drinking-driving risks.

A more complex situation to study was the introduction of lounges and taverns in Ontario which, prior to 1947, had only dull and unattractive beer parlors frequented mostly by men. The lounges allowed the sale of all types of alcoholic beverages by the drink; many provided entertainment and opportunities to dance and made special efforts to attract women and middle-class customers. Popham et al. (1976) compared trends in alcohol

statistics in Ontario and Manitoba before and after the change. Per capita sales increased more in Ontario, but drunkenness convictions and liver cirrhosis deaths increased more in Manitoba. Of course, this change did not increase availability only: On-premise drinking environments were greatly changed in terms of attractiveness.

Smart (1974b) has compared sales of alcoholic beverages in self-service and clerk-service package stores. In self-service stores, all the alcoholic beverages are displayed much as in any supermarket or clothing store. In the clerk-service stores, selections are made from a list and a clerk brings the purchase from the rear of the store. Displays are small or nonexistent. It was observed that customers in self-service stores made larger purchases than those in clerk-service stores. Interview studies indicated that self-service customers reported more impulse buying and that they reported higher average consumption during the previous week.

Several strikes among liquor store personnel have also been investigated.¹ The value of strikes is that they represent one of the few opportunities to study short reductions in availability, because most changes are toward liberalization. The best known study is the one by Makela (1974) of the Finnish Alko-personnel strike. This strike lasted 5 weeks and affected only retail outlets, not restaurants or the sale of light beer (3.7 percent alcohol or less). Effects on consumption of "average citizens" were very small; however, arrests for drunkenness and disturbances of the peace declined substantially.

Smart (1977) also studied two liquor store strikes in Canada. They were for short periods of time (4 and 9 weeks) and affected only package store sales completely, with lesser effects on hotels and restaurants. Neither of the strikes reduced total traffic accidents, impaired driving, or traffic fatalities. However, one had a substantial effect on arrests for public drunkenness. This finding shows, as does the Alko strike, that persons whose drinking is excessive and beyond control can be affected by alcohol availability. It is known that most of those arrested for drunkenness are typically socially deteriorated or skid-row alcoholics.

Some interest has been taken in changes of hours of sale. They have been a favourite means of regulation, but few controlled, empirical studies of their effects are available. Writing about wartime measures in Britain, Shadwell (1923) contended that having

¹The author has not had direct access to studies of the Swedish strike but they are believed to support the conclusions above.

shorter selling hours was the most efficacious measure employed. However, such a large variety of changes were made affecting availability that the value of any one is difficult to assess (Smart 1974b).

Popham (1962) found a correlation between opening hours of beer parlors in Toronto and arrests for drunkenness. However, the same hourly pattern of arrests obtained for Sunday when beer parlors are closed. Popham and associates suggested that "the hours of sale reflected the drinking pattern of at least one segment of the community rather than the reverse" (1976, p. 591).

Raymond (1969) studied the change in on-premise closing hours in Australia. Traditionally, taverns closed at 6 p.m. to allow workers to drink after work but still get home for dinner. This led to the "6 o'clock swill," that is, hurried drinking from about 5:30 onwards. When hours were extended to 10 p.m. in Victoria, Raymond found no effect on total personal injury accidents. However, the peak hour changed from the 6 p.m. to 7 p.m. hour to the hour from 10 to 11 p.m.

It is unfortunate that so little information exists about extending opening hours for package stores, Sunday openings, holiday closings, closings on voting days, and the like. This is particularly true because legislators are inclined to manipulate the hours and days of sale more than almost any other control measure.

From the studies discussed above, one might agree with Popham and associates that "variations in indicators of the prevalence of inebriety are not dependent on outlet frequency" (1976, p. 584). Outlet frequency also does not appear to govern average consumption. The effects of changes in hours of sale on consumption are difficult to assess. In short, small changes in availability of the type examined above typically have small or insignificant effects on drinking and drinking problems.

Large Changes in Availability

There have been many situations involving large changes in availability. Chief among these would be prohibition, wartime shortages, and the introduction of stores to previously dry areas such as in Finland.

The effects of prohibition on drinking have been well studied in the United States (Warburton 1932), Canada (Popham 1956), and Finland (Bruun et al. 1960). In all these countries, prohibition created a major reduction in per capita alcohol consumption and in such alcohol problems as drunkenness, liver cirrhosis, death

rates, and the like. Many arguments can, of course, be made against prohibition, e.g., that it encouraged illicit trade in alcohol, that it contributed to the development of gangs and criminal elements, and that it created disdain for the law. Probably all these arguments have some validity, and it therefore becomes a matter of individual judgement whether prohibition was worth the effort. It may not be worth examining its effects in detail, however, because prohibition is an unlikely policy in the present world.

A similar difficulty exists in assessing the considerable effects of wartime restrictions on drinking. Although important historically, they represent situations that can only be duplicated at intolerable costs. So many events occur during wartime that may limit drinking that it is extremely difficult to say that availability reductions were most important.

The best studied wartime restrictions were probably those enforced in Britain during the 1914-1918 war (Shadwell 1923; Smart 1974a). Shadwell concluded that "excessive drinking can be effectively checked and the disability caused by it, reduced by appropriate measures." The principal measures were seen to be curtailment of hours of sale, limitation of supply, and higher prices. All these availability factors were no doubt important, but neither the separate value of each nor the value of all availability measures can be assessed. Smart (1974a) has suggested that the following were important in the effects of wartime restrictions on drunkenness and liver cirrhosis.

1. From 1916 to 1918 restrictions were made on beverage output and importation.
2. People had less time to drink. Working hours increased from 48 hours a week to 60 hours or more in some types of industry, unemployment decreased, and many wives went to work.
3. Bombings, blackouts, and the like in large cities, especially from 1915 onwards, may have made people less willing to move outside their houses for drinking occasions or off-license purchases.
4. A general wave of patriotism perhaps led people to reduce their drinking in keeping with the examples and exhortations of Lord Kitchener and the King.
5. The war could have increased prohibitionist and moderationist feelings that had already existed before the war. In fact, per capita consumption of alcohol and drunkenness convictions passed their peaks around 1900 and were on the way down at the time of the war.
6. Shortages of policemen and physicians could have made the detection of drunkenness and liver cirrhosis deaths less likely.

More interesting studies of availability have examined the introduction of alcohol outlets to isolated areas of Finland. As in most Scandinavian countries, Finland has had a long history of temperance, with low per capita alcohol consumption and a spirits-drinking tradition. Access to alcoholic beverages is still difficult in many parts of Finland; liquor stores are difficult to find and often crowded. For many years no alcoholic beverages were sold at all in most rural areas and small towns in Finland. A limited number of stores were introduced in 1951 to market towns. Numerous comparisons of drinking and drunkenness were made between these towns and others that did not have stores (Kuusi 1957). The major findings were that the opening of stores increased the frequency of drinking, especially of beer and wine; decreased the use of illicit spirits, but had no effect on excessive drinking or drunkenness. In general, then, the opening of the stores was of uncertain social value. Kuusi's study does constitute one of the best studies of the effects of increasing the availability of beer and wine in dry areas.

Another Finnish experiment in the years 1969-1970 involved even larger changes in availability (Makela 1972). However, this experiment was more complex and more difficult to interpret. Prior to 1969, liquor stores were not allowed in rural areas, and licensed restaurants were highly restricted. Public opinion demanded liberalization of these control laws so that (1) the number of liquor stores was increased substantially (from 132 in 1968 to 167 in 1970); (2) the number of fully licensed restaurants was increased by about 80 percent; (3) some 17,431 new medium-beer (3.7 percent) shops were introduced and (4) 3,000 medium-beer bars were opened. Public intoxication was also decriminalized. The aims were to liberalize the existing laws and to make available low-alcohol beverages that would substitute for the favored spirits. It was hoped that this would control the explosive intoxication from rapid spirits drinking that was typical of Finland.

Unfortunately, total per capita consumption increased by about 48 percent in the first year. Also, the number of heavy users increased proportionately. There was little sign that beer drinking substituted for spirits drinking as expected. Beer did account for much of the increase, but there was no compensatory reduction in the use of distilled spirits. Of course, it is uncertain whether the findings from this study could be generalized to other countries with different drinking situations. Because of the rapidity of the change, it is very likely that the increased availability contributed most to the increase. It is difficult, however, to be sure that public

desire to drink more was not an important contributor interacting with increased availability.

Availability and Alcohol Use in the Future

By way of conclusion, it is worth speculating about what the future may hold for the relationships between availability and drinking. One approach is to examine worldwide trends in drinking. Sulkunen (1976) recently reported data from the World Alcohol Project (WAP) that are highly provocative. The Project involved gathering data on apparent alcohol consumption and drinking patterns from more than 30 countries over a period of about 20 years. Unfortunately, no data were gathered on the levels of availability in various countries, hence cross-national comparisons are not feasible. Nevertheless, some of Sulkunen's conclusions provide grounds for speculation. They include the following:

1. There is a worldwide rise in alcohol consumption.
2. The rise has been greatest in countries where spirits are the preferred beverage.
3. There is homogenization of drinking habits throughout the world. That is, beer is becoming more popular in wine-drinking countries; wine, in spirits-drinking countries, etc.
4. The homogenization is not at the expense of traditional beverages but in addition to them. That is, the populations of beer-drinking countries have not stopped drinking beer because they are drinking more wine and spirits. Traditional beverages are also becoming more important.

If traditional beverages are becoming more popular at the same time as nontraditional beverages, is that because they are more available? Perhaps it is more related to international travel, the coming of the global village, or other cultural events. Will these changes lead to demands for greater availability of nontraditional beverages, which in turn will create more consumption? For example, in Canada, a beer-drinking country, there are many taverns or pubs where only beer can be purchased. Will there be more demand for wine bars or wine taverns or for general licenses like those most pubs in England have? If this were the case, could we be sure what the effects on drinking or drinking problems would be?

Sulkunen has also noted that in beer and spirits countries the drinking of wine with meals has become more common. This may mean that women and young people will have more alcohol available and hence drink more often or at earlier ages. Of course, it

could also mean that the relatively high abstainer rates among these groups will decrease to those of the older male population. Perhaps, too, this change will lead to greater demands for beverages that suit young persons and women. The so-called pop wines with low alcohol content and sweet tastes are perhaps an early sign of this trend. Other demands could include sweeter spirits or cordial drinks and sweeter beers marketed for these new drinking groups. We might find too that there is a demand that nontraditional beverages (typically costly) be lowered in price.

Sulkunen has found that in low-consuming countries such as Finland, very high proportions of the total consumed are accounted for by alcoholics and other heavy drinkers. When per capita consumption increases, as it has throughout most of the world, more of the population will consume larger amounts. This will mean that the nonconsuming or low-consuming groups, e.g., women, the very young, and the elderly (60 plus) will consume more. In order for this to occur, some increases in availability will probably be necessary. Some of these might include lower legal drinking ages, youth-only bars, or special drinking places for women. It is probable that the 1969 changes in availability in Finland were in part an effort to promote drinking among women who rarely drank spirits. Certainly these changes tended to increase female drinking more than male drinking and to increase the occasions on which small amounts were drunk by infrequent drinkers.

If the current trends described by Sulkunen were to continue, it is impossible to see how they could not create a demand for greater availability. Granting such a demand could, of course, further increase per capita consumption, which could lead to further requests for more availability.

Summary and Conclusions

1. Availability can be defined in various ways, including subjective, social, physical (social control), and economic availability. Almost all research to date has been concerned with the phenomenon of physical availability, with some work on economic or price availability. Relatively little interest has been taken in how social and subjective availability affects alcohol consumption.
2. Very few efforts have been made to examine the independent effects of increased availability, not considering such influences as urbanism, affluence, the desire to drink more, and attitudes about drinking. Gross availability scores do have much unique

explanatory power when current per capita consumption and liver cirrhosis rates in the United States are examined. Before making firm conclusions, it would be best to have more cross-sectional and longitudinal studies done with availability just one of a number of possible explanatory variables.

3. Reasonably good presumptive evidence exists to show that laws lowering the legal drinking age lead to increased alcohol consumption and alcohol problems among young people. Evidence is more conclusive that such laws increase alcohol-related traffic accidents.
4. Research on most small changes in number and type of outlets and hours of sale indicates that such changes have little effect on consumption. An exception to this is probably the introduction of self-service stores.
5. Many small changes in availability have *not* been carefully examined: Sunday drinking, lengthening of hours, and introducing new pop wines for young people are just a few examples.
6. It is difficult to examine many changes in availability because they were made at the same time as others, or would be expected to have a cumulative effect. We need to examine methods of analyzing aggregations of small effects over time.
7. If availability has an independent, as opposed to an interactive, effect, this is probably true only for situations of very large change. The best example with minimum interference from other factors is probably the total prohibition enacted in various countries. Changes due to wartime restrictions are difficult to interpret.
8. The Finnish studies of increasing availability are among the most interesting but offer somewhat confusing results. The first Finnish experiment indicated relatively little effect from a major increase in availability on per capita consumption and excessive drinking. The second found many more important increases, perhaps because it examined a larger increase in outlets. At present, empirical studies give little sure indication about how large an increase in availability (however defined) must be in order to affect per capita consumption and excessive drinking.
9. Current world trends in alcohol consumption suggest that demands for greater availability, especially on the part of new consumers, may be made. If they are made, it is uncertain how they will affect consumption and whether they will lead to repeated cycles of greater per capita consumption, greater availability.

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Discussion of Paper by Smart

M. H. Brenner

This discussion raises several general theoretical and methodological issues brought to light in Reginald Smart's paper.

The Definition of Availability

In his definition of availability, Smart includes "physical" (involving formal social control through the law), "subjective" (involving personal estimates of cost in time and resources), "social" (involving the influence of small groups such as family and friends), and "economic" (involving price). Given its assumed policy relevance (and relative efficiency in data collection), in the paper for this conference Smart concentrates his empirical analysis on physical availability. In doing so, he controls statistically for the effects of income and urbanism and then finds no significant relation of availability to alcohol consumption or alcoholism.

The most general theoretical question raised by this analysis lies in the definition of availability. In this definition, there appears to be an underlying assumption of the traditional and extremely useful axiom of a supply and demand basis for alcohol consumption. Supply is regulated largely by the economics of production and government policy, with formal legal controls over "physical" availability. Because (as Smart points out) if the public desires, alcoholic beverages can be produced by individuals at a small fraction of their commercially produced cost, availability must depend largely on legal restrictions on supply and on "subjective" and "social" restrictions. In this understanding of availability, however, the demand side of the equation is largely absent. This omission proves extremely troublesome because it is probably the element of demand, as seen in the income and urbanism variables, that is in central control of alcohol consumption and related pathologies.

In fact, the demand component of consumption would appear to be as much an aspect of availability as is legal restriction. When we speak of restrictions, we must consider not only legal restrictions but also those components of the social-control process that are informal and constitute the *usual* norms or conventions by

which any society lives. Indeed, of all the controlling norms of societal behavior, legal restrictions constitute an almost infinitesimal fraction. Typically, it is only when the usual and informal norms are subverted or radically changed so as to produce harm to society that formal legal institutions of social control (legislative, civil, and criminal justice systems) are instituted or enforced. Thus, the demand for alcohol (as for any other goods or service) is largely a function of those behaviors the society promotes or restricts in terms of its usually operating normative system.

Availability, then, can be seen most generally as that which a society through its normative structure, both informal and occasionally formal, (1) promotes and (2) does not restrict. Our understanding of the extent to which alcohol is normatively available to a society will depend on our identification of the great variety of norms that bear on the desirability of alcohol consumption under different conditions. This variety of norms on alcohol consumption will relate to the social structure, including the social status and roles occupied by persons of differing age, sex, occupation, income, education, etc. Thus, the encouragement or restriction of the consumption of alcohol will depend on one's position in the society as it relates to the overall culture, including the society's values and beliefs. The questions of the proper age to begin drinking; whether pregnant women, or any women for that matter, should drink; whether it is proper for individuals of high position to drink; whether one may drink in a work situation; whether it is appropriate to drink under conditions of high or moderate stress or tension; whether intimacy or conviviality should be induced by drinking; and whether sobriety is understood as noble or as ridiculous are answered by examination of a society's norms, values, and beliefs. Availability, in a word, is almost entirely what a society considers to be *proper* at any given time.

By examining the impact of income and urbanism on alcohol consumption, Reginald Smart has utilized two of the most powerful influences on the evolution of informal and formal controls. Growth in real per capita income and in urbanization is typically understood by theorists in sociology, economics, political science, and anthropology, as the key factor in long-term social change involving culture and social structure. Because real per capita income and urbanization are predominant influences on social norms, it should come as no surprise that they outweigh all but the most powerful legal restrictions in their influence on alcohol consumption and related pathologies. Smart is therefore absolutely correct in controlling for factors that are "obvious" and over-

whelming to the social theorist but are commonly neglected in evaluative research in the field of addiction, as well as in the mental and physical health fields generally.

Moreover, Smart is probably correct in excluding alcohol prices in estimating influences in consumption since he has already entered per capita income into the equation. As others have pointed out, the proper measure of the effect of price on alcohol consumption would utilize relative price, i.e., the price of alcohol divided by the prices of goods and services generally or by disposable income. The significance of relative price is that it refers to the price of alcohol relative to other consumables or available income. Such a ratio is useful considering that there are a limited number of consumables on which disposable income can be expended, and the choice of alcohol may depend in part on its cost vis-à-vis other items or services.

If real per capita income is included in a multivariate equation that seeks to explain variation in alcohol consumption, the *relative* price of alcohol is likely to be subsumed (by the real per capita income). Because real per capita income controls for the effects of price change (inflation), the assumption is that it will probably purchase more of any consumable, including alcoholic beverages. The higher the real (disposable) per capita income is (among regions or over time), the lower the relative cost of nearly all consumables.

Significance of Income and Urbanization for Alcohol Consumption

In order to understand the pervasive influence of income and urbanization on alcohol consumption and associated pathologies, it is necessary to observe how these phenomena may affect norms associated with the culture (values and beliefs) and social structure (social institutions and social roles). Among the more important societal aspects that are subject to change under conditions of increased real per capita income and urbanization are: (1) idealational structure—scientific, religious and ethical systems; (2) demographic structure—population size, density, heterogeneity (by ethnicity and occupation), and age profile; (3) industrialization process; (4) family and friendship (i.e., small primary group) structures; (5) migration patterns; (6) educational systems; and (7) mass communications networks.

Thus, economic growth (or increased affluence) and urbanization tend to be empirically correlated with many different aspects

of social change. This is a result of both the multidimensional character of these large concepts and the fact that increased affluence and urbanism (in the causal sense) produce changes in culture and social structure. To exemplify the massiveness of these changes, an outline of probable correlates of income and urban growth are presented.

A. Economic growth

1. Real per capital income growth makes all consumable goods and services relatively cheaper.
2. Economic growth tends to be accompanied by increased leisure time (as a result of increased use of labor-saving devices). This permits increasingly larger amounts of time away from work and other responsibilities. This time is available for social gatherings, entertainment, eating and drinking at restaurants and taverns, etc.
3. The increased income itself provides greater wherewithal to indulge in leisure-time activities.
4. Economic growth brings increased levels of education, providing increasingly larger blocks of time that are free from adult work responsibilities for the students as well as a greater sophistication and cosmopolitanism in tastes and life styles.
5. The increased cosmopolitanism is accelerated by a combination of the greater income and leisure time in the form of increased travel, both intranational (especially inter-urban) and international.
6. At the same time, economic growth is frequently accompanied by economic instabilities involving, among others, unemployment and inflation.

B. Urbanization

1. Demographically, urbanization can be defined as the increase in population size, density, and heterogeneity. The increased size and density make more likely the occurrence of economic competition, crowding, and congestion. The increased heterogeneity, based on ethnic and occupational mix, gives rise to less binding normative structures. From the demographic view, then, the process of urbanization can be seen to engender considerable life stress.
2. Urbanization is also typically correlated with industrialization involving the rationalization and routinization of work patterns as well as the imposition of impersonal, bureaucratized social relationships. These phenomena are

thought by scholars to affect work satisfaction and tensions in the work setting profoundly.

3. Urbanization is also understood to involve an increasingly highly specialized labor force with tendencies toward social class divisions that produce tensions and anxieties among persons earning relatively high, as well as low, incomes.
4. With increased urbanization, several types of change in family structure tend to be found. First, there is the decline in family size and influence leading to a profound decrease in the degree to which the family shapes the behavior of youth. Indeed, family-based norms come to compete severely with those of other institutions and small groups. One also tends to find increasing age-aggregation of the population with important subcultures based entirely on age (e.g., adolescents, young adults, the middle-aged, and the elderly). One of the more important demographic implications of the influence of urbanization on family structure is the emergence of the single young adult or young couple living alone—i.e., without the influence of major social or family responsibilities. This population group tends to be highly mobile and cosmopolitan in its tastes.

When one is examining income or affluence insofar as they may affect alcohol use, it is clear that positive relationships are expected both on the ground of sociocultural changes affecting norms and social stresses. However, without a careful breakdown of the dimensions of the implications of these powerful concepts, it is difficult to understand which factor or combination of factors empirically influences alcohol use. This is not to say that the conclusions reached by Smart on these variables are incorrect, but that research utilizing these variables has clearly just begun. One is also reminded of two other factors associated with both urbanization and economic growth: (1) the enormous growth in the female labor force, and (2) the development of mass communications. While it is possible to speculate, there is scant information available on which to base a prediction of the effects of such profound changes on alcohol use.

The Significance of Epidemiological Studies for Prevention

The emphasis of policy studies through the epidemiological method is on prevention. Whereas the clinical method usually has as its objective the testing of specific chemical or surgical therapeutics, the epidemiological method seeks to ascertain the origins of disease processes that originate in the social or physical environment and are therefore differentially distributed among the population. This differential distribution of illness-producing environmental factors can be observed cross-sectionally among different populations or over time in the same population.

From the standpoint of prevention, we are then able to ascertain theoretically which illness-producing conditions are affecting which populations. This procedure in turn leads to the policy questions of the means by which illness-producing factors can be modified, curtailed, or regulated so that either they are not experienced by a population in the same quantity or their illness-producing effects might be decreased. Once such ameliorative policies have been introduced, it is possible to examine the population by survey in order to ascertain statistical differences in the incidence or prevalence of pathologies associated with the illness-producing environmental factors that theoretically have been controlled through policy interventions.

In the case of policy research vis-à-vis alcohol consumption, the epidemiological method, based on comparisons among strategic political units, represents the only practical method of policy evaluation. The major reason is that there are a significant number of interacting factors, found disproportionately among population groups, that bear on alcohol consumption and attendant pathologies. These interacting factors include culture and subculture, political system, socioeconomic class, income, education, urbanization, age-and-sex role, and social-psychological stress, among others. Thus, in order to estimate the effects of a given policy on alcohol consumption, it is necessary to control for those factors and trends that normally determine the level and character of alcohol consumption.

Cross-Sectional and Time-Series Analysis

There are essentially two basic methods for assessing the impact of policy on a program that is designed to have an impact on a sizeable population. These techniques are either of the panel-

analysis or time-series analysis type. A panel-analysis in the alcohol-consumption area might typically involve an examination of cross-sectional data among different populations at three different points in time. Thus, at the first point in time, estimates are obtained of the multivariate effects of those interacting factors which significantly affect alcohol consumption prior to the occurrence of a policy's effects on alcohol consumption.

At the second point in time, the effects of the same variables are estimated, but now include an additional estimate of the effect of the policy among one (or a few) of the populations under study. During the second point in time, we would expect to observe a change in the extent to which factors other than the new policy determine the level of alcohol consumption (including a decrease), because a new determining factor has been added. Further, we might expect that the overall level of alcohol consumption has changed from the first to the second point in time, because the policy should have taken effect.

At a third point in time, we would again observe the multivariate effects of our initially significant variables, as well as the variable introduced by means of policy, in an effort to measure the more lasting effects of the policy on alcohol consumption. For example, it might be that the effects on alcohol consumption of variables other than those related to the policy have been even further curtailed. In addition, we might observe that total alcohol consumption has been reduced even further. Or we might find the reverse—that the effects of the policy were short-lived and do not extend into the third period, or that over a longer run the policy produced measurably negative effects.

The second method, the time-series analysis, is based on the measurement of changes in trends. By this method, the researcher is able to take into account the long- and short-term historical factors that impinge upon the rate at which the population is involved in a particular activity over an extended period. By this method it is possible to ascertain the effects of a number of factors on past and future (predicted) trends in, say, alcohol consumption. This method is an important complement to the panel-analysis and is superior to it in the area of policy evaluation. In evaluation studies it is necessary not only to have baseline data as to the situation existing prior to the introduction of a new program, but also to attempt to assess what would have happened to the population behavior *without* the introduction of a new policy. It is entirely possible, for example, that the behavior change in the population would have occurred in any case and that the probability of this behavior change could readily have been pre-

dicted by continuous observation of the trend of that behavior in the population. In that sense, we would hope to observe a sharp departure *from* the trend if the new policy had the effect of altering behavioral patterns beyond what was to be expected given the population's tendencies already in evidence.

In recent years, time-series analytic methods have been further developed to the point where it is possible to ascertain which variables are actually affecting the movement of the trends under observation. With the use of these techniques we are able to predict with much greater accuracy the probability of change in a given behavioral trend. These methodological developments have far-reaching implications for policy evaluation.

In the case of alcohol-consumption trends, we are able to identify at least four major groups of factors that at any time substantially determine the rate of persistence in consumption at a specific level. These groups of factors include the availability of alcoholic beverages (supply), the demand for alcoholic beverages in light of their culturally understood and sanctioned uses, the level of tension or stress in the society, and prevalent policies governing availability and actual consumption. As with the cross-sectional method, the time-series analytic method would attempt to incorporate all relevant variables that ordinarily impinge on alcohol consumption, in order to account for the existence of current trends in consumption. Then only departures from the effects of these variables would influence the movement of alcohol-consumption trends if new policies were to be implemented. Similarly, it is possible to analyze any specific historical trend in alcohol consumption in light of policy changes, given information for the same historical period about those variables that would ordinarily influence the consumption trends. Thus, through a combination of statistical and historical research methods, we are able to gain an understanding of at least the most important factors that bear on the behavioral effects of implemented policies.

Necessity for a Multivariate Scheme in Evaluation of Alcohol Consumption Patterns

There is today a very large literature of international scope, developed over a period of 60 or more years, on the effects of a variety of different variables on alcohol consumption and attendant problems (see appendix). These studies have dealt with age, sex, income, urbanization, and prices, among many other factors (including scattered elements of government policy) that might

influence alcohol consumption and related pathologies. With very few exceptions, however, the literature does not indicate sustained study of the interacting effects of policy considerations with sociodemographic, sociocultural, or psychological variables. Rather, what we find are studies that concentrate on individual variables, the effects of which may ultimately be either greatly suppressed or greatly exaggerated due to the influence of other significant variables not included in the studies.

It is clear that any serious examination of government policies at the present time, when multivariate techniques are available to us, must take account of the interaction of factors encompassed in each of the other major areas—supply of and demand for alcohol and tension-producing factors that are indices of each of the three areas in a *single* model and research design. Indeed, studies that are not grounded in such multivariate models may lead to extremely misleading results. At best they may exaggerate the influence of a few significant variables, but at worst they may neglect entirely the influence of several crucial variables.

Determination of Costs and Benefits

In an assessment of the costs and benefits of policies dealing with regulation of alcohol consumption it is especially necessary to take account of the multiple interacting factors that ordinarily influence the consumption rate. There are two outstanding reasons for this. The most obvious of these is that factors that ordinarily influence consumption may either enhance or diminish the effect of the policy. Thus, during periods of high stress or the development of a vogue for consumption of a particular alcoholic beverage, the cost may be extraordinarily high in terms of the degree of regulation required by a restrictive policy.

The second reason is that given curtailment of the use of one type of alcoholic beverage, the usual multivariate factors affecting consumption may influence the population toward the utilization of different beverages or perhaps other intoxicants, depressants, or even stimulants. In this case, the effect of the policy may be quite adverse in that the use of alternative methods of stress-reduction may be considerably more harmful to health and perhaps to criminally defined pathological behavior. In the latter case, the benefit side of the cost/benefit equation may be sharply curtailed or even made negative.

The more systematic and thorough cost/benefit analysis of policies affecting alcohol consumption must therefore take ac-

count of the multiple factors that *ordinarily affect* consumption, as well as of those that *result from* the policies themselves. Of particular importance in policies that seek to control behavior are those latent behaviors that are initiated in response to the initial suppression of the behavior under control. This type of detailed cost/benefit analysis therefore requires an understanding of those factors that initially influence the behavior one seeks to control as well as *each significant alternative behavior*. If it is hypothesized, for example, that decreased use of alcohol might lead to increased use of cigarettes or coffee, then one must examine those factors which ordinarily lead to alcohol, cigarette, and coffee consumption. If the conditions leading to use of each of these substances are similar, then one must calculate the extent to which they are substitutes for one another (among the same subpopulations). The historical-statistical method then becomes extremely useful in observing consumption trends in each of the substances under conditions of varying supply and policy regulation.

Summary and Research Suggestions

1. Both theoretical considerations and empirical findings point to the relative insignificance of small-to-intermediate policy changes on physical availability of alcohol in relation to alcohol consumption and associated pathologies. This is especially true when one controls for the major factors that ordinarily influence normative patterns of alcohol use.
2. Even from a policy standpoint, therefore, it is evidently important to research far more thoroughly into the usual and more powerful sociocultural influences on alcohol use. This is true both because we want to control for their influence in evaluation research and because policy can be based on those norm-controlling factors themselves. Such policies include using the mass media for health education that may have a far greater potential for moderating alcohol abuse than reduction of physical availability of alcohol. These policies may also be better suited to the political process in a democratic society. Relative successes of mass media-based health education have been shown in the community mental health, malignancy, and cardiovascular disease fields.
3. In evaluation research based on statistical methods, it is necessary to utilize models that are sufficiently complete for the control of *all* major influences on the dependent variable (in this case, alcohol consumption and related pathologies). A step

toward the construction of such a model has been attempted in this discussion.

4. Ideally, the most sophisticated techniques of time-oriented statistical analysis should be used in this type of evaluation research. At a minimum, multiple panel-analyses should be undertaken. Time-series multiple-regression analysis would appear to be most appropriate to the alcohol problem, however, because it is necessary to consider variables with time trends of varying duration as well as lagged relationships.
5. In addition to utilizing a scale of physical availability of alcoholic beverages (with factors equally weighted), it would be useful to know whether individual factors or clusters of factors have a significant impact on alcohol use.
6. In policy-evaluation analysis of alcohol abuse, it is necessary to use several different indices of alcohol use. Identical policies and sociodemographic factors may have different implications for different outcome measures—cirrhosis, cirrhosis mortality, psychosis related to alcohol abuse, alcohol habits, alcohol-related traffic accidents or deaths, alcohol-related family violence, alcohol-related cardiovascular illness or malignancy, alcohol-related maternal and child health problems, etc. It may also be important to utilize outcome measures that are not related to alcohol abuse but that may be increased if alcohol availability is curtailed, such as use of over-the-counter medically prescribed, or illicit drugs.

General Discussion

Paul Whitehead: I want to address myself to one of the points Harvey made: worldwide trends in consumption and our limited capacity to alter them.

I guess I agree that our capacity to alter them is somewhat limited, but it seems to me that that is all the more reason why we have to be prepared to take up those opportunities that present themselves. My expectation is that the largest trends in per capita consumption in North America are going to be the result of increasing the use of beer and wine with meals. That is because very few people regularly use beer and wine with meals in North America. So here the role of advertising in spreading that particular practice is important to look at. So are methods of marketing, for instance, the possible importance of self-service liquor stores. There are several jurisdictions in North America that don't have them. Many of these jurisdictions are in the process of introducing them, and there is a need for more studies on the effect of self-service liquor stores not only on purchases, but also on consumption and patterns of consumption of alcoholic beverages.

Particularly important here also, some mention was made about pop wines. I don't know of any studies on pop wines. We did one small study related to low-alcohol-content beer. Our findings indicate there that low-alcohol content beer tends to result in some people using that beverage in a situation where they formerly used no alcoholic beverage—people beginning to drink beer who did not drink beer previously. It leads to an expansion of their drinking repertoire and is therefore more likely to lead to increases in per capita consumption. If we have any opportunities at all to intervene or slow down these worldwide trends in some jurisdictions, it seems to me that it is apt to be in these areas. So we have certain types of targets to aim at.

Robin Room: It seems to me that the extension Dr. Smart proposed of looking at subjective and social availability is very important. That involves stretching the concept of availability so that it includes a good deal of what is going on under other titles already in the literature in terms of contextual studies of drinking and the norms of drinking. Certainly subjective and social availability is another way of talking about the normative structure surrounding drinking. And it also becomes a way of including the mind in the discussions so that we are simply sneaking in the mind under the rubric of what has been traditionally an economist's term. This is really an argument about words. The crucial point in Dr. Smart's

discussion is that matters of legal availability have to be seen in a wider context.

I think that a couple of comments on Dr. Brenner's presentation should be made. It seems to me that Dr. Brenner's methods and the application of econometric methods to new fields of data are important, interesting, and may hold some implications for policy. But I would question whether they should be seen as superior to other methods for policy assessment. I think that different methods have different virtues from the viewpoint of policy as well as from the viewpoint of research or other aims. The superiority of econometric methods in policy assessment may arise primarily from the fact that economists have been closer to the centers of power than other social scientists, so that those in power are better able to accept an argument made in econometric terms than one using other social science techniques.

I think it is important to see that while time-series analysis does have unique capabilities and certainly implies a lot more about change (which is fundamental to understanding policy) than a cross-sectional analysis does, there are limits to time-series analyses that have to be recognized. It is assumed, for instance, that there is a homogeneity in broad terms across time that breaks down under certain circumstances. If you did an analysis of public drunkenness arrests or of some mental hospital commitments for alcoholics in the United States that went into the 1960's and early 1970's, then you would find that the fundamental meaning of that indicator had changed, and you would have to put it down as a discontinuity in the data and put it in as a dummy variable in one form or another. The insertion of dummy variables like that to take account of realities will point to the essential arbitrariness of the econometric models under some circumstances in which you really try the model and see which one fits best. In a lot of econometric research you try several different models, and fundamentally the choice between them is which one seems to work best with the data. I think these are powerful and useful methods. Dr. Brenner made a substantial contribution in bringing them to bear in the new area, but I would be wary of claims of their superiority for policy assessment over other methods.

It seems to me we are in danger of falling into the same kind of methods imperialism that we have had with the social sciences for a number of years, in which we fall into what I regard as futile battles over whether observational studies, or survey-research studies, or one or another kind of methodology is going to be the most powerful tool we can bring to bear on research questions. One thing that particularly worries me about the

analysis is the large distance there often is between the indicators used in the very powerful econometric techniques and the assumptions made about them in the interpretation.

For instance, I am not very enamored of the concept of stress, which seems to have a strong existence in the sociological literature. I think Doug Parker and Marsha Harman referred to an article in the *American Journal of Sociology* this morning that uses a global concept like that to interpret time-series data on variables that are rather far removed from being a direct measurement of it. Very often, it seems to me, these global variables are labels to cover our ignorance of the actual structure of behavior that underlies it. It seems to me that on the one hand you can say that, as Dr. Brenner says, it is no surprise that Dr. Smart's data came out the way it did. On the other hand, it is also very easy to say it would be no surprise for it to come out the other way. When Dr. Brenner discusses why his findings concerning the effects of urbanization and income are no surprise, we could think of reasons for it to be logical to come out any way. In other words, we are very quickly back in the world of post hoc interpretations of why the data came out that way.

I think the methods are very powerful; I think the methods are very implicative; I think the methods can test the things we have left in the realm of conjecture. But we have to listen clearly as we use the methods to the point of inflection between when we are describing what is on the table and what can be seen, and when we are interpreting the data in terms of the general miasma of available social science concepts.

One other comment. It seems to me that with respect to availability there is a fundamental issue that, at least in the United States, underlies a lot of our discussions about what we are going to do about the social handling of alcohol with respect to availability. It is the issue of whether we are going to try to integrate it into our everyday lives or whether we are going to try to maintain it as separate and see it as different. There's a kind of irony that Wolf was pointing out this morning that from his perspective alcohol should be looked at econometrically just like any other commodity. On the other hand, we might want to say that there is some point in maintaining alcohol separately in people's conceptions—that it isn't immediately obvious whether the better social strategy for reducing alcohol-related problems is to integrate it into our everyday lives (in the supermarket as well as in a separate liquor store) or whether there may be some point in keeping alcohol as a somewhat separate, special substance, thought of in different terms from taking a drink of water and treated

differently in our provisions for its availability. Really, our society is somewhere between the possible solutions on this. They didn't give us any alcohol with the tea we just had. In Holland at the International Conference on Alcoholism and Drug Dependence, one of the things I noticed with interest in the convention hall was that soft drinks were the most expensive; coffee, less expensive; beer, the cheapest.

Harvey Brenner: On this notion of imperialism in connection with method, the assumption is that this is a very peculiar technique in some ways. It is the same regression analysis used in cross-sectional studies. There is no fundamental difference at all. Identical tests of significance are used, identical methods of data, etc., all the components we have learned about in cross-sectional regression analysis apply here. The same structural equation modeling occurs in cross-sectional analyses. In fact, there has been a lot more of that, as you know, in the last 10 or 15 years (at least among sociologists and economists) than has occurred with time-series. The only significance, the sole significance of time-series, is that it allows us to take advantage of historical experience. What is the importance of that?

It is that if an evaluation study is performed only at a certain moment in time we have no idea what are the peculiarities of that era or whether that phenomena or surrounding phenomena at that moment in time had an impact on the situation. The value and the superiority (and I do believe substantial superiority) of time-series over other time-oriented techniques simply expands sample size, and in any case expansion of sample size makes the technique superior. (I am afraid I must use the term superior despite the lack of etiquette in use of such a term.) It is simply a way of examining time-oriented relationships. As to being able post hoc to do whatever explanation one can, this is true of cross-sectional work. Well, it's true of nearly any kind of design, except perhaps highly experimental designs in which one can control the acts of these nonhuman populations.

You are perfectly right. The technique is dummy-variable technique—at least to begin with. But it need not be, because there is another method that allows us to incorporate the cross-sectional with the time series. For example, supposing there is a breakage in time. If we examine the different States, each one, two, or three times separately, studying the effect of unemployment (varied across the States in terms of, say, its implications for mental hospital admissions), we would now want to explain that phenomena. We could do a cross-section of that. We simply enter the time-oriented relationships into a new cross-sectional study.

There is nothing particularly sacred about the technique. All it does is take the usual, in this case regression, methods and invert them. Instead of using as a sample size persons or units that are aggregates of persons, such as States or cities, we make them instead units of time—days, minutes, hours, years, whatever.

Griffith Edwards: I would like to take up the question of stress raised by Robin. Unemployment is not a social fact or personal experience in its own right. It depends on how you interpret the experience. Unemployment is experienced in a social and personal context, as is drinking. If I am unemployed and born to unemployment, there is no great stress as it is quite enjoyable living on a free income. If I am unemployed because I am a woman, that used to be acceptable, but it is no longer quite so enjoyable because unemployed women are under some pressure from their sisters. To retire to a village where you are honored and sit in front of an oak tree, growing wiser as you grow older and older, is not too bad. Whereas to be retired and have your secretary taken away and all your identities robbed—that makes unemployment unpleasant. We should try to see behind Brenner's graphs to these questions. Who are the newly unemployed in times of rising unemployment? The impact of unemployment will depend very much on the welfare system. In Britain there is the poverty trap. People who are unemployed are receiving higher wages than if they were employed. Although we now have an unemployment level that is very worrying, it is by no means as socially disruptive as it was in the 1920's and 1930's. People are not being broken.

I would be worried, therefore, if Harvey's findings were accidentally interpreted as a new given law. I know this is not his intention. I would like to see what the social implications are that lie behind those interesting empirical findings. Is it the unemployed who kill themselves? Is it the long-term or short-term unemployed? Is it professionals who have lost their jobs? Or is it the wives of the professionals who have lost their jobs? There is an increase in the suicide rate under 15 years of age that needs some sort of explanation.

Certainly in my own country the quality of first-aid psychiatric services is still woeful. It is possible to have gross and treatable depressive illness in, say, working class women that goes untreated. There is evidence that drug treatment, properly used, would reduce suicide rates. I would like to have Harvey's comments on the sociocultural context.

I wonder also who it is that dies of cirrhosis in these conditions. First and foremost, one might wonder if it is necessarily the

person with alcoholic cirrhotic problems. Wolf has taken crude cirrhosis death rates rather than trying to segment out alcoholic cirrhosis, which is difficult. But the fact that he has gotten away with it in certain types of analysis does not mean we can always get away with it. One obvious factor is that there may be a change in nutritional standards with unemployment, and this may result in the sudden demise of people who were just keeping going. I'd like to know whether Harvey has further information that allows us to see the social and personal significance of subgroups.

Harvey Brenner: There is no question that the intensity of the variable—unemployment for instance—will vary with the subgroups of the population. The experience of unemployment may be very, very different. There has never been any implication to argue the reverse in the sense that one asserts something awful, if you will, about the entity of unemployment. There are no laws, as we understand in the physical sciences, that pertain to anything in the social world, because these independent variables, as well as dependent variables, change in meaning and as such, through time.

There has been a change in the character of unemployment altogether in the last 10 or 15 years. Yet there have been downward shifts in the economy, lowering income for certain groups by age, sex, race, occupation, etc. It is important to know which groups go through that experience; which groups respond in that destructive way. In the case of suicide, and by the way the meaning of suicide—suicide is not the same for a Swede, is not the same for a Japanese as for an American, etc.—yet death itself is identical.

When you look at something like suicide you find a fault. You find the shape of a curve that is parabolic, in which the middle-aged male is most sensitive and the very old and the very young are least sensitive. In the case of women, until World War II there was little relationship between their suicide rate and changes in employment. Since World War II, the relationship has come to be nearly equal to that of males. This is despite the fact that females have much lower rates of suicide, but higher rates of suicide attempts. This is just an example of the kind of research that is possible with the same set of general procedures. It is possible to analyze these more and more and finally to include more and more variables to help illuminate the process. This is extremely important and helps us to find where the crucial causal links are and where the proper areas of inadication might ultimately be.

Wolfgang Schmidt: I find analysis that distinguishes long-term from short-term variation extremely useful; it brings out differences that would not emerge at all if only the usual long-term

analysis was performed. For example, if I remember your report correctly, you showed that the short-term response to economic stress reveals itself in an increase in spirits consumption. This response is immediate and is eventually also reflected in arrests for drunkenness.

Now that leads you to interpret the increase in spirits consumption as a stress-management device. It is interesting that in a similar study in Finland, different conclusions were reached. During periods similar to your economic-stress periods, they found an immediate reduction in consumption and drunkenness. They offered as explanation for their findings that, during economic-stress periods, people find themselves powerless, highly vulnerable. They cannot afford to take the risks involved in getting drunk. When the economic situation improves, the response is an increase in both drinking and drunkenness. Apparently drinkers feel that under these improved conditions drinking is relatively safe.

The point I want to make is that if we are correct in these two interpretations, we might find an interesting cultural difference in the response to economic stress.

Robert Straus: Yesterday we were stressing primarily the effectiveness of qualitative research in helping prepare us for quantitative research—in identifying what the questions are by asking potential subjects to help us identify the questions. With respect to some of the very questions raised in the last half-hour, we could very effectively use personal-interview techniques and diary techniques to determine the meaning of changes in alcohol consumption at the level of individual human experience.

How does unemployment affect alcohol consumption? We have heard several different interpretations. Let's get some studies. Let's go out and ask people. I have talked to a lot of people who are suffering from what we call in Kentucky "Appalachian Syndrome." The Appalachian Syndrome involves an incredibly vicious spiral. It involves undereducation, unemployability, high rates of drinking, high rates of reproduction, high rates of morbidity, and one could go on. There is an overwhelming sense of futility, tied to a large amount of somatic symptomatology. We need both these techniques. We need to recognize that urbanization, for some people, means concentrations of population, traffic jams, waiting to be seated at a restaurant. We can conjecture about the use of alcohol to help people cope with such stress. But these kinds of questions can best be answered by getting subjects who will either share with us through fairly intensive kinds of interviewing or who will themselves learn to keep matter of fact, routine records of their feelings and the everyday experiences (the

baby is sick; momma had an accident, menstruation was painful) they encounter.

Finally, I want to ask whether one factor associated with the increase in alcohol consumption is the increase in the rate of change itself and its impact on human beings. The sense of obsolescence, the lack of security people feel in what they are doing today and its ability to sustain them throughout their life—I think this is a factor that has coincided with the rising rates of alcohol consumption throughout the world. This factor has largely been precipitous in the last 15 or 20 years. It is also a factor in what we consider to be the generation gap and the communications gap. With some imagination and with suggestions from the subjects of qualitative research, we could design some indices that would enable us to test the relationships between changes in such diverse but related factors as employment security, lifestyle, food consumed outside the home, and alcohol consumption.

Richard Jessor: I am concerned with the issue of drawing causal inference from these kinds of data. The question, I think, is about the role of time-series analyses in identifying covariation. And looking at these macroindicators as important covariants of particular dependent variables is worth looking at. The problem that is raised concerns intervention. That is, policy really has to get beyond covariants to deal with the ideas about the mediating variables and the kinds of theory one supposes link up these two variables. Robin expressed his lack of commitment to a stress notion, and Griffith balanced that off with his interest in stress. What I think is at issue here is the possibility of working in this way, somewhat more theoretically; to do that would really mean that one needs to explicate—to make much more overt—what theoretical presuppositions one is entertaining along with the demonstration of the covariations. Doing that may well serve to guide one toward looking at different dependent variables.

As I look at that whole array of variables, I feel they form a package—they tend to be a package—that might in some way have a kind of entity quality to them. They are all negative, problematic behaviors. Now generally in psychological analyses of correlation designs we tend to use another kind of technique, which I think would be important (not only methodologically but in terms of ferreting out which theoretical model might be appropriate), and that is what we call discriminant validation. So that, for example, at the same time one documents a covariation between unemployment and suicide rates it would be important to look at other kinds of differences of high frequency. Does your fear of stress say that suicide ought to show the same fluctuation, that is,

a consonance over time? Does it also say that church attendance ought to increase? Now I use this because it isn't an indicator, a dependent variable in the scheme, and yet it has the possibility of either jeopardizing the stress theory or providing what we might call a far-separated kind of indicant—something that isn't part of the package of suicide, heavy drinking, violence, aggression, and so forth.

It has both theoretical implications and methodological implications in so far as you can show that your variable covaries with those things it should and does not covary with those things it should not. That latter demonstration of discriminant validity is crucial. The theoretical point is that it forces you then to array a large set of indicators, some of which would be consonant with a stress theory and some of which might be consonant with a powerlessness theory or some such thing. I wonder if in your own work you have had time, interest, or inclination to think along this discriminant validity kind of approach as a way of testing theory?

Harvey Brenner: The answer is yes, 10 years of it. Looking at this approach, many of the chronic diseases (particularly the cardiovascular group) respond in somewhat the same ways. There are some illnesses for which mortality is unrelated. Now for the issue raised regarding the linking variable—regardless of what research we do from now until the species is discontinued, the question of validity will always remain in any piece of research we ever do, whether in biochemistry or in physics. The gaps are never closed. There is no end to filling in the missing pieces of linkage that must be filled in. We are just at the beginning of a field that will evolve as time passes. The same question follows, as Cavan pointed out earlier today, for the distribution model: What is it that links the presence of the stuff or its price to consumption? Let's say that someone suggests a good variable for that. Then the question will be: Why is that variable behaving that way? Somebody will try to explain that, and generally, we hope, somebody will try to explain what makes the subsequent link. In a word, the causal chains are endless. At any point in the causal sequence, from a policy standpoint, one ought to be able to intradict the process. The nice thing about it, this kind of theory building, interpolative theory building, is to locate from the humane standpoint the most efficient source of intradicting the process, if that is possible.

Reginald Smart: There are a few small points I want to mention. I want to say something about relative prices—looking at availability and alcohol consumption—things of that sort. It turns out that in some States, apparently, you can't even get relative prices, and you can't get prices of alcoholic beverages unless you

are looking at the control States. You can't determine the price in a place like California, and you can't really determine it in many other places. You can get enormous ranges; you can make guesses on taxes. It turns out to be rather messy—except for the 17 control States where prices are fixed. Some States do not make these prices accessible.

As we did our work, it seemed to me that if overall availability scores are not very important in predicting alcohol consumption when income and urbanism are controlled, then it wouldn't be profitable to bother with other sorts of variables. If you have essentially a no-result situation, it would not seem profitable to me to go into a whole variety of other situations, unless it is primarily alcohol consumption you are interested in. Now in the work we were doing, we were interested in availability scores. We weren't really interested in knowing what predicts alcohol consumption. I think, too, that if we were to go about it in a big way, it would be at the risk of introducing every variable for which we could get State data—without really having a clear concept of why such a variable was being introduced other than it happened to be on hand. There should be some sort of theory or some sort of rationale or expectation for picking out the variables one would pick.

Alan Williams: I am concerned with the assumption that springs up toward the end of Dr. Brenner's paper—that mass media campaigns, public information campaigns, have great potential for moderating alcohol abuse. When empirical evaluations have been done of mass-media campaigns—and there have been very few such evaluations even though these are available techniques—there is typically either no effect on behavior, or in some cases there have even been adverse effects. For example, there was a recent media campaign to increase family planning; it was found that more unwanted pregnancies resulted. There are other examples of adverse effects from such campaigns in alcohol and drug areas. I don't mean to say that the findings are all negative; they're not. Yet I am convinced, on the basis of research evidence, that mass media campaigns are not the answer to changing behavior.

Larry Wallack: The whole issue of mass communications is one of the most commonly used models for the prevention of alcohol problems. It was pointed out that some of these campaigns have actually backfired. I am not sure about that being the case; some of that evidence is anecdotal—as far as some of the smoking evidence that is used. But I do think the evidence for mass communication's being effective and changing behavior is one thing when incremental behaviors rather than decremental behaviors are being

pushed, whereas the results from using mass media alone—even including a number of things beyond the electronic media—to promote decremental behavior or to eliminate a behavior from a set of behaviors have been dismal. There is not a lot out there to support optimism. Joining mass media communications with different types of networks on the community level is a different thing. This is something that the people in the cardiovascular study mentioned here have been somewhat successful in doing. I think, though, that as a blanket sort of approach, mass communications is not the way to go—it is just not substantiated.

Richard Jessor: I would like to make one point that is related to Reg's conceptualization of availability and to his awareness that availability can be conceived in a variety of ways. His focus was on physical availability, but he makes reference to social availability and, I think the term was, subjective availability. We use the notion of perceived availability—that people's behavior in drinking or using alcohol is most proximally affected by perceived availability.

Thinking in terms of perceived availability raises the whole question of alcohol advertising. The issue of advertising as a component of the availability issue is critical; it really bears looking into. We are all aware of concerns about advertising, but if one thinks of advertising as an aspect of the availability issue it becomes a very critical component which suggests that, with physical availability constant, you can have enormous variation in perceived availability as a function of advertising or messages that constantly remind people of the fact that alcohol is out there. I am surprised that no one else today seemed to mention the issue. I think it is socially an arena that requires a good deal of our concern.

Joy Moser: Well—I have been tremendously interested in the wide range of topics discussed here, but I am also tremendously interested in the conspiracy of silence, or whispering behind the hand, about one topic that seems to me very germane to the question of availability, and that is the control of production. Now I thought that the research discussed here was supposed to impinge in some way on what was going to be done to further prevention of alcohol-related problems. I fully understand that it may be exceedingly difficult to control production in North America, but I find it very strange, nevertheless, that one doesn't even look at possibilities of control.

In looking at possibilities of control for North America, one might want to look at what has been done in history, or what is being done now in some other countries. I think that one might

also want to look at the history of what has gone wrong when production was not controlled, and there I think the recent book by Lynn Pan is absolutely fascinating. She talks about what happened under colonialism in Africa, how the tremendous increase in drinking, particularly along the coast of West Africa, was a direct result of imperialist trading, and how the fighting between the imperialist powers was very often about whether they should put taxes onto each other's alcoholic importation into countries.

Here I would like to mention another dimension of availability, that it involves not only production but also importation and exportation. And if you are interested in international affairs, I think that at some point you would want to give some consideration to possibilities of controlling importation and exportation. Now most countries seem to put controls on imports to safeguard their own production. What about controls on exports? There seems to be very little control internationally on the exportation of alcoholic beverages. Now, can we think a little wider than just our own country—to the international significance of uncontrolled exportation of alcoholic beverages? Is it internationally feasible that international bodies should be allowed to set up big breweries in developing countries, to set up new distilleries? I mention this because in 1975 at the World Health Assembly a resolution was passed stating that many governments had noted with great concern the considerable increases in consumption of alcoholic beverages and what they considered to be related increases in health problems. And there was a demand that more careful consideration be given to doing something about these matters.

What does doing something about these matters mean? We really don't know much about it. Should we look at history—should we, as Lynn Pan did, look into the history of what happened with narcotics, and was what happened of any use at all in fact in controlling international trade? Is it at all feasible that researchers like yourselves should take an interest in the international aspects of increase in production? Obviously, the increase in consumption doesn't occur without increase in production—unless you have enormous stocks somewhere.

Some interesting work has been started in connection with the Common Market in Europe. France, as you know, has become very concerned about its high consumption of alcohol and is one of very few countries where consumption levels have decreased in recent years. Now one can't see any direct reason for this decrease in consumption. One reason may be that they have a national body that attempts to look at causes for high consumption; it may merely mean that the French are turning to other things,

that young people in France are taking more drugs or are adopting different drinking patterns. At least they have some controls on consumption in France now. One thing that is worrying them tremendously is that, with the Common Market and the increase in free trade, these controls may break down completely. I don't know how much that affects you here in North America, but you may want to give some consideration to the possibilities of free trade between other American countries. I think there is a Latin American common market, isn't there? As far as I understood (although I have very little information on it), the main objective is to increase the availability of consumption of alcoholic beverages.

Now I would like to mention another point, that when the matter of world alcohol statistics was discussed there seemed to be a certain complacency about the fact that a number of statistics had been amassed. But these statistics were merely statistics on registered production and importation, and exportation and stocks. Now the majority of the world does not buy its alcoholic beverages in shops. It produces them at home. Again, we know very little about the patterns of production of alcoholic beverages and the changes that are occurring.

If I might give an example from a country with which we are working rather closely at the moment—you may know that the World Health Organization is trying to give increasing attention to the developing countries, and I merely speak now in the hope that you may have some interest in the fate of developing countries that are changing so rapidly. I would like to mention a statement by a man who is working in Zambia and is working very closely with us. In 1964, when they attained independence, the Zambians had access to imported alcoholic beverages for the first time, and it is reported that consumption went up very rapidly. At the same time, Zambians began to have access to jobs that were formerly held by expatriots. The result was increased alcohol consumption and increased car ownership; the two came together and brought sudden increases in other problems related to alcohol consumption. Well, I would just like to say that I do hope that sometime in the near future there would be some rather careful consideration of what possibilities there are—not necessarily of decreasing production—but of doing what the Canadians are trying to do—stabilize availability. I don't see how you are going to stabilize availability unless you stabilize production.

Theoretical Synthesis: Discussion

Griffith Edwards

All I can say is that you must be joking if you generously suppose that anyone could have the power to attempt a synthesis of what has gone on in this very rich feast of ideas. I feel that you have really set me up as some sort of jester because you know I am going to fail. You can have no other expectation. So I would ask you to see that the merit in what I say will lie in my failure. I think that we all will fail, and by watching me fail you can have, as it were, a preliminary view of the probable impossibility of bringing all of these ideas together satisfactorily. I hope that is a successful preemptive strike to explain to you that when I slip on a banana skin I really meant to do it, and that is part of the act.

What I always tend to do in my own mind when faced with overwhelming complexity, with papers dealing with matters beyond my professional expertise, set in a culture that is not my own—what I tend to do, quite frankly, is to go home. That is what I mean to do tomorrow. I decided that that was really too long to wait, and that I would probably go home now. I can only grope with, deal with, all this alienation you have thrust upon me by going home this instant. This is what I mean to do—at least in my imagination. I will escape from all this and go back to my own home, my own town, my own real world, and reflect on what I have heard away from all this academic excellence. I believe that this is good for the soul, to take what we have been told and go out and look at our own.

It is in that town, or that stretch of countryside, and not in this room that our academic ideas must ultimately be tested and where we are going to see if they have any strength. What I will do, therefore, is climb to the top of a hill in the park near my house. With the time difference, it must now be late afternoon, and with any luck, it will be sunny atop that hill. You look north over the Thames, which bends at that point. You see the vast expanse of the east end of London, the poor quarter; poor it has always been. You see the cranes rising from dockland. You look over Greenwich itself; you see the Queen's house and the splen-

dors of Inigo Jones. Then you look at the tower blocks of the city housing, rising among the slums. You look in another direction, and you see an area of London preserved with some of its former elegance—a middle-class enclave.

If I stood on that hill, I would see all these aspects of present complexity; but that is only a start. I would have to note that over there is a West Indian immigration, and that's complex. Then I would have to note that there was an Indian and a Pakistani immigration, and that is curious too. It is not a minority group of poverty. These hard working people are often carrying on with their traditional skills of trading. They are buying up tobacco shops and little corner businesses as they had previously done when they moved to other parts of the world.

Now I say all of this not for just the fun of it, not just because I want to go home, but because I need to remind myself of the extraordinary complexity of this society in front of us. I have to say then, "Well, what were they telling me all those days in the room with the white tablecloths?" I wonder whether I have forgotten this real world while I listened to them. When they were talking about norms, well, whose norms? This Pakistani who just bought the shop, who runs his family on the rigid lines my Victorian great-grandfather would hardly have dared, whose life is a system of absolutes? Or were they referring to that Irish family in the Tower block with too many children, greatly overcrowded, with a breakdown of whatever system existed in their own countryside when they gave up their farms to come over here? I wonder about that old lady, the widow of the distinguished artist, living in that beautiful house there, drinking her sherry before dinner, but more than that, I wonder about her notion of her importance and certainty, her own rules of conduct. Or am I talking about that young women in the next house, married to the successful business executive, awash with money, awash with drink, and with very few and uncertain values.

On top of the hill, I would say that it was extraordinarily unlikely that one solution could be totally effective for the problems of all of these groups. I am not saying anything that wasn't said in the meeting, because Dick was telling me about the Indian community he studied; Bob mentioned the Appalachian syndrome. Of course that awareness was there. I wonder whether we translate that awareness too easily into the visible separateness of the American Indians or the Appalachians, rather than seeing all the complexity that lies in the one street—that there are no such very certain changes as the norms of even one suburb.

And looking over from the hill, looking over the Royal Hos-

pital, I would be aware that some social scientists, who shall remain nameless, have done a drinking survey in a community nearby, fairly representative of the rundown corners of South London. They have shown extraordinary class differences in drinking practices; for the typical working class (using that phrase as shorthand for the Register General's Classification IV, V), the pattern is largely of drinking beer with rather little wine and occasional spirits as a treat. The professional and managerial classes (I, II) are far more eclectic in their choice of drink. You can really tell the socioeconomic position of a man simply by looking at his drinking profile.

The working class man tended to drink at weekends. The weekend was really a fiesta: Friday, pay day, to the pub; Saturday, lunchtime; Saturday evening; Sunday, lunchtime—if there was still enough money for it. Of course, no stereotype is true; this was only a typical median behavior. For Classes I, II, a day without a drink would be quite unusual. Their drinks would be a bit heavier over the weekend. And one would see that the working class man would do most of his drinking in the pub. Indeed, even someone who had advanced to a severe alcohol dependence, an "honest working man," quite often would keep some sense of integrity and worth by saying, "But I never drank indoors." Drinking indoors, keeping his cap on indoors, or swearing indoors in front of women would be the ultimate degradation. The home is simply not the place for drinking. Social Class I, II man drank in various settings. Sometimes he drank at the pub at lunchtime with some business friends, but he drank at home very often and, indeed, swore at home in front of women—a middle-class norm.

When we look at class motivations for drinking, to some extent they overlap. They all knew that alcohol is a psychoactive drug, even though they didn't phrase it thus. Both groups used alcohol to quell bad nerves, nasty feelings, boredom, tension, anger—to get feeling right. But the working class men also drank for something that can be called "clubability." Drinking was so much within the setting of the pub and the pub itself was so important that you really couldn't separate the motivation for drinking and the motivation for attendance at the pub. They liked the dominoes; they liked the darts; they liked the social exchange; they liked standing and exchanging and receiving drinks. The motivation for drinking was embedded in these social practices and this symbolism.

The Social Class I, II man liked to drink with meals, and preferred to drink in this more intimate social setting. He liked the taste of drink, and he liked a dry sherry instead of a sweet sherry.

He would prefer to buy a French wine instead of a full-blooded Spanish Beaujolais. In a way, he was drinking more like a Frenchman than an Anglo-Saxon.

Now if one looked at the women in the two groups, the Social Class I, II woman drank in many ways rather like her husband. She would probably have a drink every day too. She drank in her home. She had the same motivations; she had the same eclectic tastes. But there was a very considerable gap between the drinking of the IV, V working class man and the woman. Here, the man really hogged the drinking. The woman was most likely to go several nights or even a week or two without a drink. She might occasionally be taken to the pub with him. But all class relationships were again shot through with age interactions. No doubt the younger males leaned more towards a homogenized pattern, and the younger females (no matter what class) tended to drink more like the corresponding male.

Whose norms count here? We have only begun to touch on the real complexity of this. The Indians, Pakistanis, West Indians, the old lady who is the widow of the artist? There are a lot of people there whom we haven't seen even from the top of the hill. We can see the extraordinary complexity, contradictoriness, and variety of ideas on norms. Whose norms are to dominate? But you will tell me, "No one ever had such an idea as dominance. We are all culturally sensitive." Good old us.

I do notice that it is always us teaching the Indians and the Pakistanis, rather than the other way around. I notice a steady stream of white, middle-aged, middle-class males jumping onto airplanes to provide consulting services to Asia or black Africa, and very few Asians or black Africans coming to provide consulting services for this country. This is not meant as a cheap or careless remark. There are many things to learn from other cultures, which we neglect at our peril. We should really see how an Orthodox Muslim country manages to get along without drinking.

Even so, with those groups (when I look down from the sunny hill), I am really at a loss to know how I can propose norms for the white males, let alone the females. If I check on this quarter in the literature on education put out by the humane people who take the responsibility for the drinking education in our country, they have sought to impose middle-class norms as national norms—like the BBC, which wouldn't let people broadcast without wearing a dinner jacket, or read the news without speaking a castrated version of the Queen's English. The middle class was purveyed by the BBC; the middle-class norms are in many countries purveyed by the alcohol educators.

I don't know what is happening in the U.S.A. You haven't got a European type of class system here. But one hears the same things said: "Well, you ought to drink with meals, and you ought to drink in the home. It's the place to do it. You should drink frequently, but lightly. You shouldn't get boozed up out of your mind on Saturday night." But one might ask, "Why not?" It seems to be an extremely culturally integrated notion to get boozed out of your mind on Saturday night, provided there is a pub or two on every street corner (then you can walk home rather than having to drive home) and provided too that you are sensible and don't spend too much money on it. Note that there is no evidence really that in the IV, V group there is an excess of drinking problems compared with other groups.

What am I doing in proposing the notion that we should all wear dinner jackets and speak attenuated Queen's English, and drink like English executives? You will rightly tell me that I exaggerate, and that none of my fears (as I stand upon the hill) are other than frightful fantasies; that no one has ever shown this degree of cultural insensitivity; and what I am doing is simply portraying a lampoon which you will courteously, as ever, forgive me, knowing that the English are a funny lot at best of times.

I just wonder what examples could be arrayed of successful, preventive intervention on the basis of the sociocultural model. I put that out as a friendly challenge—expecting that a little later on a whole lot of hands will catch the chairman's eye as a whole lot of instances are brought to mind of successful intervention based on sociocultural models. It is just a failing of my memory that I cannot immediately, myself, put up both arms. I can think of a few examples of intervention that seem to have worked. Father Matthew's crusade, for instance, was, I believe, splendidly effective. He didn't have a research grant. He didn't write a program. He didn't even hold a workshop. He wasn't a task force. He got up there and he told them to stop drinking. And it was culturally congruent; he was not an imported English health educator who was a fully trained alcoholologist. He was an Irish parish priest, which the world badly needs. He knew his people. What he said fitted into his knowledge of their suffering. What he said fitted into his religion and their religion.

Prohibition was a piece of social engineering which we should perhaps study more closely in order to understand what really happened. Prohibition is to me still very much a puzzle. But certainly it was not a program based on the findings of research workers. It really is extremely difficult to find examples of the successful application of that great body of sensitive observation

and splendid theory—the sociological and anthropological accumulation of knowledge and wisdom on drinking. I have enormous difficulty here because I so much enjoy that literature. It has been scientifically exciting while it has been thoroughly humane. It has been one example of society examining its workings—exciting literature to which I think many people ought to be directed. But I fear that the application of those findings to effective, preventive action still seems to set such large problems.

But as we look again over the suburbs sprawling below that hill, we should also be aware of its history. If I were to reach down from my library shelves the liquor-consumption figures from 1885 to 1935, I would find that there was a difference between maximum and minimum per capita alcohol consumption of 100 percent. It is a rough calculation. People were willing to halve their alcohol consumption over a recent historical time. It appears that the difference between the highest and the lowest level of alcohol-related deaths was a factor of four. Liquor consumption was halved; associated deaths were cut to a quarter.

Now, I like to look over that Park into its history. I like the fact that the Park was once the Royal Garden and Henry VIII's jousting ground. Where the knights would meet in mock combat is now the park where my children play. I am fascinated, though, not just by those grand events in history, but by the fact that over a fairly recent period people were willing to cut their liquor consumption in half. There is proof that liquor consumption is not God-given and absolute. It is, very obviously, at the play of forces. And I want to know why. We all want to know why.

Similarly, I want to know why once over there was Gin Lane. And now it is gone. I want to know why it was once respectable to have street fights. Older residents can remember that in Greenwich (at the bottom of the hill), before the war, women would come out of their homes and start fighting. It was an acceptable let-off of emotional steam.

I want to know why small elements of culture, small segmented aspects of drinking, have changed. I want also to know why in 1834—a little after the Reform Act—Buckingham produced a Parliamentary report on drinking, with the problems then (as so often since) seen as peculiarly those of the labouring poor (or a minority group). That Committee came forth with a series of small recommendations—that there should be public parks, that there should be cafes where people could go and take the family without drink, that people should not be paid their wages in pubs, that part of the pay should not be given in terms of liquor. It was a series of small solutions, intimately illuminated by evidence as to the work-

ings of sectors of that society. I note too, that on that committee there was no medical representative.

In the 1870's they had a new committee—this time with heavy medical representation—and the suggestion that the solution was the setting up of inebriates' reformatories for dealing with the individual person rather than with society.

Those Tower blocks, the problems of the emerging Pakistani shopkeeping class, the problems of women of wealth and leisure, women cramped into the city housing, or people who are suffering from depressive illness, or the itinerate Irish or Scottish laborer who fills our labour force almost as a disposable person—these are the present realities and they will not be met by solutions that ignore their variety.

I would like to see the social scientist not demoted, but invited modestly to attack segmented problems rather than to propose spurious national norms, or connive at the mounting of television campaigns that are expensive, insensitive to subcultural realities, and doomed to fail. The place of the social scientist has to be redefined. Social scientists must be asked to address themselves more modestly to more segmented and delineated problems, and must get close to other peoples' realities. They must do this with every method available, feasible, and relevant (whether it be ethnographic, or survey, or econometric). They should regard themselves as people with a bag of tools—working men and women.

Looking out again, I think I see someone else stalking through the streets. It is General Booth. A man who walked down streets, saw, recorded, analyzed. He was a social scientist, but he was also an agent provocateur, a preacher, a social innovator—a man who wrote *In Darkest England and the Way Out*, and tried to set up the way out. There was a close connection there between social science and social action. I take Don's warnings; we don't want the streets crowded with General Booths and nobody else, but I do believe that the general is a great example to us, and I would like to see the connection between social science and social action made much closer. I think the social scientist should be challenged to take responsibility for seeing things through, rather than just doing the survey. I always felt one of the weaknesses of social science compared with medicine is that doctors, at least, treat their patients rather than write essays on them. I believe social scientists should also have this responsibility imposed upon them. Otherwise, I believe their lives are luxurious.

Now as regards what else I see in that community, I'm about to come down from that hill and retreat to that comfortable draw-

ing room, look at the books, look back on the days I have had in California. Before I come down I would note that the liquor consumption did fall over those years between 1885 and 1935, and that the casualty rate did come down. Certainly, employment conditions did change, the number of pubs on the street corners changed, and a lot of other things changed. But I rather think if I went up the road and talked to my friend who works on leukemia, and I told him that there was a ghost of a hint of a chance that altering the availability of some substance might cut the leukemia rate in quarter, he would be all stations go. He would be leaping into action. It would be the most incredible news. It would be a breakthrough; it would be a red alert. It would be medical research councils rushing to spend funds. It would be men dropping their present work at their benches. It would be television, radio, I don't know—a conference at Buckingham Palace.

But tell people what happened between 1885 and 1935, they say, "Oh yes," and they get on with their meeting. It just isn't noticed. I think that we need to look at the news afresh. And I think that subsequent generations will condemn us for scandalous negligence if there is information such as Wolf has given us and we just go on with our meeting. It behooves us either to accept what he says as provisionally true, or to reject it. If it is provisionally acceptable, we must follow through its implications. Otherwise, we will have heard that dirty water gives cholera and we will have said, "Oh yes, very interesting," and gone on with the day's work. If we have findings of that potential importance, with that degree of solid work lying behind it, we can only expect to be condemned if we then agree to hold another and another conference, and throw at that work small footnotes of doubt, while accepting so much else that is much less credible.

The two-pronged approach, the acceptance of both models and the actions they propose, is what is really needed. But the sociologist, the culturist, whatever he calls himself—had better start looking at things really within his grasp. A touch more modesty, a willingness to commit himself to actions, seeing things through, a study of that group there over toward the docks, or that line of houses. We need to look into communities rather than to pretend that we can look into nations. I believe that we have to be more particulate, but at the same time it ought to be possible, with an approach based on distribution, to work across the board. The sociologist must again come in to work with us on the acceptance of the distribution message and on the power structures that may resist it. The honest resistances and doubts, of course, have to be met.

Now, as to synthesis, I don't know what synthesis in this regard means. How many examples have there been of successful synthesis of theories? I thought that again I would achieve immediate acclaim if I could bring in the notion of synthesis of Newtonian physics with post-Newtonian physics. But as I can never even remember which law of thermodynamics is which, I thought that would be risky. One needs to look at the general question of how profitable it is to attempt to synthesize. Then I cheered up remembering some words of H.G. Wells that you can never get too excited about the death of capitalism if you never believed that capitalism had ever really been alive. Now, I am not sure I can get excited about the synthesis of these two models, as I am not sure whether these two models have ever separately existed.

What we can do in the long run is to try to see when consumption fluctuates, which groups are affected—whether it is the heavier drinkers, or the light drinkers, or this or that segment of society.

Well, I told you that I would fail; and I think it must now be manifest that I have done so. I've ducked the issue. And I think it is time therefore to come down from that pleasant hill. I'll sit back in my own drawing room, with thoroughly pleasurable memories of this meeting and much gratitude for being allowed to be a visitor here—the stimulation, the richness of ideas, and I think, the particular quality of interchange.

General Discussion

Dwight Heath: I would like to address the specific question that Grif threw before the group: What successful intervention has been offered on the basis of the sociocultural model? And respond with an example that I am familiar with. The Navaho Indian tribe, the White Mountain Apaches, the Kaska in Alaska, the Papago in Arizona. There are a great number of native American and native Alaskan communities and tribes with which, in fact, significant progress has been made in alcoholism, in alcohol problems, on the basis of an in-depth understanding of the local situation. These examples, I think, illustrate the point that came up about midway. I was terribly concerned, in the first part of the discussion, that sociologists or social scientists in general were being taken to task for something that most of us don't pretend ever to do. And I agree totally that the role of the sociocultural model is best to address small problems with a segmented aspect, given a local set of tools.

As you were looking over this magnificent panorama from the hill garden, you may not have seen the sociologists or ethnographers who were, in fact, finding out what are the norms of your Pakistanis, and your business man's wife next door, and the Irish working people in the slum. And in finding out the norms of these groups, they also find out what are the problems as they see them, back to this question of what are the actual problems. And if you are going to have successful attempts to address problems, you have to find out what these problems are and should be able to address various constituencies, audiences, or what have you, in terms that are meaningful to them. And this is what we were talking about when we talked about norms. Not that there is a national kind of norm; but that various reference groups, significant meaningful populations, have various norms and should be approached in a variety of ways.

I think in that sense the complementarity of the sociocultural and the distribution-of-consumption models comes out. This distribution of consumption thing is logically compelling; it seems to fit a great number of data. But it strikes me almost as a kind of a black box. And maybe if we are to understand, to explain, to account for what is happening within the black box, that is where we get into sociocultural factors—the kinds of things that Reg was talking about as familial availability, subjective availability, and so forth. This is the real relationship in terms that Bob Straus stressed yesterday as complementary, not necessarily merging in a synthesis.

Sherri Cavan: I want to speak to the concerns that Harvey raised in his presentation yesterday afternoon and that Joy Moser raised at the end of the session. Perhaps in a sense I found her comments more provocative than anything I had heard through the course of the conference. Dr. Edwards gave the example of the halving of consumption rates in England from approximately 1900 to 1935. What he did not summarize is that obviously some very important economic, political, and social changes must have been going on at the same time. For the rates of consumption of alcohol are not unrelated to a network of interrelated factors; they do not shift in a vacuum—they shift in an interrelated texture.

What I have not seen in the context of the discussion at this conference, when concern is focused specifically on affecting the mean consumption rate, is any true acknowledgment that is embedded in a lot of larger network changes that are taking place, in a sense, in a postindustrial mass society. Unemployment rates and inflation are not increasing, particularly in North America, the United States. In the last few years in our society, perhaps in the past decade, we have been faced with a political system we don't understand—beginning, in a sense, with the assassination of John F. Kennedy and continuing with its nadir, I suppose, in Watergate, in the context of government deception. We have witnessed the growth of multinational corporations that begin to change the quality of community life and in and of themselves are the "normative structures" of subcultures. We've seen the growth of television, which will replace the old forms of socializing that existed prior to the electronic explosion.

So in a sense there have been a phenomenal number of massive social changes during which our drinking patterns are evaluated. And it is not at all clear that one can talk in a reformed way of how one affects those drinking practices without seeing how they are related to those more macroscopic forces and how they are reflected in the microscopic features of subcultural variation.

Paul Whitehead: It seems to me that one of the very important things that occurred at this conference was, or is, the acknowledgement of the narrow sociocultural model—that what we need to do is to have more integrated drinking practices in various parts of daily life. And if we all agree on what appropriate drinking practices are, then we will somehow have lower rates of damage. That model is not vital; we have sort of agreed to that. Now maybe it had died before. One of the things I find somewhat curious is, to some extent, a bit of denial that it ever existed.

The idea of national drinking norms as the direction in which to proceed is not something that I or other people made up. It is

there to be found in the work of the cooperative commissions and the work of people who came later. It was not the figment of someone's imagination; it was there. It was a real life policy suggestion made—and believed in—by people of rather sizable reputation, of rather considerable power. Some remnants of it linger. But that sociocultural model that dominated so much thinking and so much action in this area finds itself, interestingly, at this conference without any supporters. And you might think the fact is that it's based on some errors, and that it totally lacks empirical support.

Robin Room: I guess I will enter a dissenting note at that point. I think the sociocultural model was successful, in one way of looking at it. The drifts of thought we see reflected at this conference are reflecting larger changes in society's consciousness.

In a wide variety of industrialized countries where people wanted to integrate alcohol more into daily life, there was a period (now drawing to a close) after the end of the Second World War in which the problems of social disruption surrounding drinking arose partly because the subcultural world of heavy drinkers was kept relatively separate, because there was a lot of disruption around the subcultural boundary, and because the heavy drinker in the drier areas of the country was somewhat in the status of a heroin user in American culture—a member of an embattled subculture at odds with the larger community. And there was some feeling that the social disruption that was associated with heavy drinking, particularly in the drier areas, would be ameliorated or lessened by integrating drinking more into everyday life.

I think that that feeling, which may not have been well articulated but was reflected in the thoughts of the Cooperative Commission, was pursued for a period now of 30 years or so. But as we solve one social problem, we always see another one appearing. And we are now beginning to see that there are some costs associated with the integration of drinking into everyday life and that the costs are beginning to be apparent in chronic health problems. So that I see the two models we're being offered here, and the concerns and focuses they have had, as reflecting two different stages in the general drift of thought and sentiment in the United States. We are now coming to the end of a stage, not only in the United States but also in Scandinavia and a number of other industrialized countries, where people are saying, "Now wait a minute." Maybe we need to call a moratorium, as Dr. Smart was saying, on further liberalization, further integration of alcohol into our everyday life. Do we really want to be in the position of France at the end? Maybe we need to pay some attention to

what's happening in terms of the physical changes that take place in a heavy-drinking society.

I think it is important for us to try to understand the process by which change occurs. I think Dr. Brenner was contrasting Grif's eloquent plea for small changes with the big change that Grif told us had occurred in England. But I think big changes happen as a result of a lot of small changes. It was always an illusion, and a very poor model of how social change occurs, to have someone stand up and announce that there were now national norms on drinking, and that anyone who got drunk four times a year was a problem drinker. We can't establish a normative consensus, applicable across the enormous diversity Grif was describing, by working from the top down. That is not, in my conception, how social change occurs.

If you look in some detail at what happened in England between 1900 and 1935, as Dr. Smart has in the paper reviewing the experience of the Central Liquor Control Board in England during the First World War, you see that there were a lot of very pragmatic people doing much of what Grif was describing in the 1930's, but in a wartime situation in which they had enormous powers as long as they didn't exercise them too strongly. They made a lot of little changes. There were a lot of small and ameliorative changes in peoples' conditions along the dimensions Sherri was asking us to pay attention to. Some specific things pertaining to alcohol were engaged in as purposive action by governmental groups through that period. My sense is that a lot of changes occurred in the context of the First World War and then never got undone. That seems to be a common pattern in societies.

So social change as I see it is often a matter of small bites adding up to something in the end. I think that in a way we have to say that a society gets the problems it deserves. The rate of alcohol problems in American society is no accident. It is a rate that is more or less what we can tolerate. It may be a little more than we can tolerate, and so the society will pay people to do something about trying to reduce it. But there is a certain sense in which we're a society that does tolerate a casualty rate at a certain level, in pursuit of other aims—in pursuit of getting coal out of the ground in spite of the accident rate in coal mines; in pursuit of maintaining individual liberty and freedom; in pursuit of the pleasures some people get from alcohol. Each of us may draw a different balance, but in the end politics means that a net balance is being drawn. The balancing of costs and problems against benefits is a continual process. And the changes that occur in our sense of that balance are small changes that add up over time.

It is clear that the casualty rate that is tolerated differs in our society for different circumstances. For instance, Chauncey Starr wrote a paper in *Science* around 1970 in which it was noted that the casualty rate in private aviation is about 100 times the casualty rate in commercial aviation. It is clear that that difference reflects different perceived circumstances. People driving their own airplanes presumably risk only their own necks and those of people who are known to them. And our society sees that quite differently from the risk taken by commercial pilots who have never met the people sitting in the back of the plane.

Thus, we make a very fine differentiation in our personal lives, and as a society, between what casualties we will tolerate under different circumstances. We need to recognize that, little as we like it, and shocking as it is, when the kids from high school get killed on the highway our society may not be willing to pay the costs involved in preventing any kid from ever getting killed on the highway in a drunk-driving accident.

I think one of the things we need to do as we begin to come to grips with more modest aims is to array the alcohol-related problems that are of concern to the society and talk about which of them it's most important to move in on first—which of them we have the best hope of alleviating and what strategies we can bring to bear on them.

And I think there will be some disagreement around the table as we talk about this. I think Dick Jessor was saying, sometime early in the conference, that he was not sure that cirrhosis, for instance, would be very high on his priority list, while for Schmidt, cirrhosis would have very high priority. We need also to look realistically at the available strategies. Is it really a lack of knowledge we are talking about when we try to change people's norms or understandings around alcohol problems? Most of us know about the consequences, or the potential consequences, of drunk driving but still carry on with it because of the normative structure or because of pressures on us of one sort or another. We need to examine whether, in fact, knowledge or skill learning, or whatever it may be, is really going to make much difference in our behavior; or whether there are ways to structure the situation so that the cost is not so tremendous. We need to consider whether air bags in automobiles will better protect the drunk driver who tends to do rather worse in auto crashes than other people do in terms of reaction to the second crash.

I think that it is important to note that when we are talking about changes in human behavior like this, we are talking not only at the level of pragmatic effects but also at the level of symbol-

ism. Symbolism is an important element of social action to keep in mind. The scientist can't expect to get away with a technocratic solution in society. If we are going to be raising taxes substantially on alcohol, that itself must be a political process argued out by the people as a political process. Even if our own politics would allow it, we are not going to get away, in this society, with making any substantial changes in policy without going through the heat of the political process of argument about aims, goals, relative costs, and benefits for society.

Reginald Smart: I wanted to follow a bit on Robin's point about what happened in England in, say, the period of 1885 to 1930. I am certainly not an expert on this period, and I am sure Griffith is well aware of most of the things I am going to say. But I would be reluctant for us to consider that period of time as a sort of mystery. I am sure that Griffith doesn't feel that it is.

From my reading of the literature, the strongest explanation is probably that there were great increases in government action in the alcohol-control field. As I recall it, the first law with any teeth in it that was passed by the central government was passed in about 1870. That whole period of time, I think, is a time when there was a great deal of control exercised over the way alcohol was sold, or who could buy it. There were new changes in age laws. During the war there were of course restrictions in supply; prices went up. Laws were changed to make it almost impossible for people to buy rounds or to treat other people. And many of these things lasted well beyond the war, from my own reading. There were also shortened hours of sale during the war, and they lasted a long time after the war, too. Perhaps they still are on now.

From data I've seen, I think there are comparable changes for the United States and for Canada that seem to relate to the period of time when the central government really seemed to get control over the alcohol-control mechanisms, which early had been in the hand of municipalities—when local magistrates seemed to have control over who got a license and how alcohol could be sold. There seemed to be a "gin lane" sort of situation both in Britain and Canada, and I think probably the United States. My guess is that restrictions on availability would turn out to be a strong contender as an explanation, not to the exclusion of other sorts of social or cultural explanations, but my guess is that restrictions would be important. I think Grif is certainly looking into that area.

My second point is a short one. And that is, I am still mystified by why it is that we have so little reluctance to bring our results on prevention to decision makers. And I do really come back to

this point, time and time again. It seems to me an abdication of responsibility, not to press things about prevention on legislators. There is no other reason for doing them. Presumably we don't do them because it's basic science, because one can't build a great deal on it. We do it in order to help politicians make up their minds. I am also surprised that politicians, or legislators, or deputy ministers—important civil servants—are almost never found at meetings like this. I think we could have done with one or two. Perhaps they attend other meetings of a comparable type. There are now some very bright deputy ministers, politicians around; they're rare, but I think some of them could benefit from meetings like this.

I don't think there's much research on what legislators need and what they want to have, in this sort of area. The only research I know of is a very interesting piece written by Blum and I think Funkhouser quite a few years back—I think it was based primarily on California's politicians. What I recall from that study was that legislators really expected social scientists to come forward with what they want to have in this sort of area. The only research I find the right person to advise them on this or that social issue. They didn't expect to have to identify who was the expert in the prevention of alcoholism in California. They expected that such people would approach them, would bring their findings to them, and would try to explain them.

Often, perhaps, we are waiting to be consulted. And from the small amount of evidence I have available, we are not going to be consulted. We are going to have to go and say, "We have this important news for you, and I am sure you will be glad to get it." And I think frequently they won't be glad to get it, but sometimes they will be.

Robert Straus: I would like to indulge in a little attempt at discussing the sociocultural perspective. I think it is important for us to recognize that the sociocultural model has existed side by side with, and perhaps even emerged from, a pathology model. And if we go back far enough, concerns about the problems of drinking were not related to drinking in a family or drinking in a community, but were related to drinking on the frontier, or drinking by disruptive elements of the community or drinking by people who were isolated from family. (Take, for example, merchant seamen, lumbermen, railroad construction workers, miners.) And what we now call the temperance movement actually was an attempt to control the disruptive drinking of people who were considered a threat to the good community—not only because of their drinking, but because of many other characteristics associated with drunkenness.

Also, I would like to remind the conference that in the beginning of the modern era of scientific studies on alcohol we were terribly biased by the fact that almost all our subjects were very selected groups of people. There were the arrested inebriates or the people whose alcohol problems happened to get them into mental hospitals, where they were a captive population for study. To go back to the early years of the *Quarterly Journal of Studies on Alcohol*, the early 1940's—nearly all the studies that presented characteristics of alcoholics were presenting characteristics of people who were labeled as alcoholics because of very specific kinds of personal traits that made them visible and subject to study. And after we moved from the captive populations, we moved to the populations of people whose alcoholism had become so severe that it brought them into another kind of visibility, either through their seeking help from AA or through their seeking treatment. I mention that because I think it has very much influenced the way our—what we are calling sociocultural—approaches to conceptualized alcohol problems started. We started our sociocultural perspective with these biases—really the biases of what we were able to study, or what we selected to study, and how we went about it.

The second point I would like to make, and these tie together, pertains to the Cooperative Commission Report, and one of many recommendations that came out of that report that had to do with lowering the age of drinking. First, I would like to suggest that the actual generation of this recommendation took place more than 15 years ago; the report was published in 1966 and was based primarily on the considered judgment of people who were reviewing studies made up to 10 years earlier than that.

One of the major influences was the drinking-in-college study. The drinking-in-college study found, among other things, a very high relationship between the negative sanctions against drinking and the probability that people who drink in the face of those sanctions would be high consumers and would have a high rate of problems associated with drinking. As I recall, while only 50 percent of the male students in the highest anti-alcohol-sanction schools drank, half of them were having problems. Whereas in the low-sanction schools, 90 percent of the male students were drinking but only 4 percent were having problems. There were also high school studies showing similar relationships, and there were some not very good studies based on comparisons of wet and dry areas.

Incidentally, there is a great deal of compatibility between these suggestions and what we are being told about consumption. What we saw was that problems were associated with a high consumption that took place in the face of negative sanctions. I remember

we talked about Mississippi and how much conspicuous consumption took place when people drove down to the Gulf Coast, bought carloads of liquor, brought it back, and were preoccupied with procurement, supply, and consumption.

There is an historical relevance here. The major problem with the recommendation to lower the drinking age was not that it wasn't a reasonable recommendation for the time, although it was necessarily based on data that were a few years old when the recommendations were formulated. The problem was one I mentioned yesterday, the rapidity of change. Unfortunately those associated with that recommendation could not foresee that the very slight increases in per capita consumption that were beginning to show up were not just normal fluctuations, but were the beginning of a trend that would bring us to a point where today we realize that in 15 years there has been a 33 percent increase in per capita consumption of alcohol in the United States. There have been larger increases in other countries. That was one thing we didn't foresee.

Nor did we foresee the lowering of the age at which children were going to begin to engage in many kinds of activities that were not sanctioned by the older generation; we did not foresee that many kinds of problem behavior were going to start at 14, 15, and 16 years of age rather than 17, 18, and 19. I don't think we foresaw what was happening already due to the impact of television, increased leisure time, the increasing association of alcohol consumption with leisure activities, and the effect of rapid technological change on the individual sense of security. All of these factors, I have to think, are very important today in relation to problem drinking. But the one thing we foresaw less than anything else was that all these factors would change at an accelerated rate simultaneously.

Now, this says to me that Grif is right when he is telling us to look at smaller aspects of the problem, rather than global aspects of the problem. I am more comfortable with that because I think in trying to look globally, as we did in recommending lowering the drinking age, we extended our expertise too far. We had to use 10-year-old data in generating a recommendation that took 4 more years to implement. We must have the courage to relate our findings, to present our convictions, but with a degree of modesty that is consistent with the limited range of our own vision.

Sherri Cavan: I do not think we can advance the well-being of our environment by continuing to speak in terms of small bits and pieces of information without ever attempting to grasp the overall interrelated system in which those bits of information are embedded. We have, as it were, an integration such as Aaron Cicourel

was talking about—about the micro and the macro. How do you integrate your position as a concerned citizen with your position as a compassionate, objective social scientist? One of the things that characterizes the perspective of everyday life—"uneducated, or not so highly educated layman's perspective"—is its very parochial nature. The fact is that it thinks only in terms of small bits of information that are immediately relative to it. That certainly if the price of alcohol goes up in a particular community, it means that fewer people will drink, or fewer people will drink more, or whatever. It is a very small kind of picture.

I would suggest that if we are going to appear embedded in the issue of policy, one way we can use our cosmopolitan expertise as educated, thoughtful, contemplative social scientists is by beginning to grasp what the larger picture is. When Dr. Straus talked about what some of the consequences were of lowering the drinking age, he spoke in terms of the fact that we were using outmoded information, etc. But what he did not bring to bear was that in that very moment, that very sociocultural historical context in which the drinking age was lowered, other equally dramatic social changes were going on. So that we do not know, in any clear way, that the end result that is perceived as only lowering the drinking age was necessarily related in that way; it is part of a much larger network.

The sense I get is that we are discussing problems of alcohol abuse at this conference, and someplace else 30 scientists are meeting to talk about drug abuse, and we don't see the link between the two. It strikes me as being a phenomenal dispersion of energy and intelligence in terms of small bits of information and in terms of avoiding the question of synthesis—looking at synthesis as though it were a banana peel on the roadway to be avoided. I think that we, in a sense, do ourselves, our discipline, and the planet as a whole, a disservice by trying to keep our problem small and conceivably limited.

Richard Jessor: To talk about the normative model, or the sociocultural model, as if it is something we can have or not, would be like talking about gravity. We have to start from the primitive position that we are sociocultural beings, or social-physical beings, whatever term one wants to use, and try to arrive at a model that enables us to work.

It would be extraordinary if we were to deny the palpable fact before us: that the way we live our lives, how we use our own personal resources, is largely affected by our norms, our sociocultural definitions, and so on. What our problem seems to be is how to get hold of that in some way so that we can be self-conscious and deliberative about its use.

The point I am trying to make is we have been surrounded historically, and we are surrounded now cross-sectionally, by enormous variations that reflect the operation of sociocultural forces. The question is how to get hold of those in such a way that we can deliberatively and self-consciously manipulate them.

In our brief paper we tried to say that the availability model is simply an aspect of the larger sociocultural system, and I think this is a point Professor Seeley made. But I want to emphasize the connectedness in another way. To talk about changing availability as if one could do this without considering the reverberations through a structure of norms, consensus, and so on, would, it seems to me, be a very mechanical and very limited way to approach that problem.

I want to remind the conference again that there are other ways to enter into the sociocultural system. Some of them are micro; some of them are macro. But I want again to emphasize the issue about opportunity. To a large extent, when one talks about opportunity, one is talking about all sorts of other things. One is talking about jobs, about education. But not just having a job and having money—having a job organizes social life in very important ways, and the reverberations are important.

Let me turn to a final point here. I think that there is a distinction to be made between research and intervention. It may well be that research needs to be small, but intervention needs to be large. That is, it may well be that we can establish models for prevention research only in a small compass. That we can aggregate some models across different types of activities, and that once we have done this (in terms of prevention research), then it may well be that the way to saturate the social system is to move to a macro level. That is, it may well be that if we studied schools and found that black kids in schools were not developing in the way they should, and there weren't whites in the school, it may well be that that kind of understanding from school studies may lead to national legislation about the integration of schools. That is, there is no reason to say that because the studies we may be capable of doing have to be done on a small scale, they may not lead to national legislation or large macro action that may have a larger effect.

The reason I want to emphasize this point is because prevention, if it is done on a small scale, may not be successful. It is almost impossible in a contemporary society to insulate a local situation from the larger context. Not only is it difficult to insulate it, but it is difficult to forestall the homogenization that is brought about by the mass media, which transcends any kinds of geographic boundaries.

Well, the point is, once we have some understanding about perhaps small and beautiful prevention studies, then it may well mean that the implications need to be carried out at a level that will subtend a large variety of communities so that one has a large enough effect and a large enough insulation from counterfactors.

Dwight Heath: I think the one thing that would be a grave problem would be if in embracing the distribution-of-consumption model as perhaps the best means of large-scale prevention—if in doing that we were to throw out the values of a sociocultural perspective or sociocultural approaches, then I think we would be in real trouble.

Coming back to Dr. Seeley's other imagery of changing the gradient of the ski slope versus instructing or helping the skiers, I don't think these are necessarily mutually exclusive. I think you could change the slope, and at the same time try to find out what problems the various skiers have and try to address yourself to those in the same way Jessor is talking about—the micro level of research work—perhaps adding up to providing some kind of macrolevel strategy. That does not exclude the possibility, and I would submit the feasibility, of simultaneously having microlevel strategies that are addressed to the problems of portions of the population. We are back in the business of looking at "either-or" when I think we could much more fruitfully be looking at "both-and." In terms of immediate large scale macro impact there is no question in my mind that changing availability would have a more immediate and a larger impact than the kind of uniform monolithic national approach that has been used.

I feel that even such an approach could be much stronger if it were complemented with specialized educational campaigns addressed to particular constituencies. And we know from experiences in mass media whether we speak of advertising in a commercial sense, of educational, noncommercial advertising, of advertising in an education-commercial sense, or whatever; I think they're very similar. Even with the mass media we find that advertisers do address themselves to selective audiences in very different ways. They do this on a cost accounting basis, and they feel that it is worthwhile. I think that we should follow that cue and attempt to understand—that is what I meant the first day when I talked about the inside view—what is important to particular populations, and address ourselves to that.

Harvey Brenner: I would like to elaborate on the interesting idea that research may need to be small but intervention large, on the ground that if we intervene small we may not see the effects of the larger system that may intervene because the smaller

systems do not operate in isolation. Quite right. Exactly the same argument, however, must be made on behalf of the research. One cannot often observe, all together, the changes that occur looking at the bits and pieces without the larger story. As Sherri Cavan pointed out, it is logically necessary to have a systems understanding of the normative social structural characteristics of the changing society in order to understand how any part of it works.

A very good example of that, a very direct succinct example of that, was provided in Dr. Smart's paper. He has a scale of physical accessibility that shows in itself an interesting, significant relationship to alcohol-consumption problems. But when he controlled for one variable, the victim, that relationship disappeared. Now it is possible that given other kinds of principles there might have been some effect of one or more of the kinds of physical availability measures. But in order to do that, one would require a larger scale understanding of the general kinds of factors that usually impinge on alcohol consumption and alcohol-related problems which may in some ways be different, depending on the particular subpopulations.

I think one cannot distinguish between the small look and the large look. One has to have both and almost simultaneously. One has to look at the subcomponents of the aggregate, because the aggregate makes no sense at all. The aggregate population is not sensible, intelligible, unless we understand its components. We have to understand sex differences if we want to understand the increase in alcohol consumption. We have to understand migration patterns and subcultural movements if we want to understand the aggregate as the whole. But we must understand the aggregate as a whole, otherwise the programs that were interested in policy are going to be meaningless as well. Both must be simultaneously understood.

As to whether there is any evidence for the successful practice of public health education vis-à-vis mass media, there is lots of evidence, all over the place. And it is not only—and this was very well put by Dr. Jessor—a matter of nondeliberative manipulative behavior on the parts of controlling forces, that is, forces that have some access to media control. There are very deliberate campaigns at work all the time. We refer to them as propaganda if we don't like them or are very frightened of them. When we feel relatively neutral about them, we refer to them as advertising. Advertising markets virtually everything we eat, the way we dress. Many of our social relations occur through those media, which are very skillfully marketed, very carefully researched, developed to at least as high an art form as any of the practices in our social

science research. People who do this work generally are graduated from the same institutions that teach and perform research. So let us forget the notion of the ineffectuality of deliberate propaganda, advertising, whatever. This is the way our society ordinarily runs.

Now as to some interesting examples of public health efforts in this area, let me give you just a couple. We have a model of alcoholism as illness. Where did it come from? It was quite deliberately propagandized, very well advertised, and carefully screened until it was adopted. That is in the field of alcohol.

Let's look at cardiovascular disease. A few years ago the Heart and Lung Association put out a booklet on risk factors and heart disease, stressing the issue of cholesterol and blood blockage, and asked that people with demonstrated cardiovascular disease cut down on their egg consumption. So potent was this message that within a year the egg industry was in a crisis. So severe was this crisis that to combat it the egg industry (and this was written up in *Business Week*, among other professional magazines) hired two or three major advertisers and came out with slogans like "the egg is sexy" in an effort to reorder the public's point of view about consumption of eggs. Ladies and gentlemen, the egg industry is still at it—just keep up with the advertising literature (those of you who care to), and you will see that it is really quite serious. This is also true of the butter industry. Margarine has made a major dent in lubricants for breads and in cooking oils, and so on, in large measure due to the activity of the American Heart Association.

Screening for hypertension has become a major issue in American society and again to a large degree due to the activities of the Heart and Lung people. One cannot fail to notice at the same time the incredible programs of physical fitness we have begun to see all over the United States within the last year and a half, with all kinds of people of every age attempting to engage in some form of physical exercise.

Marsha Harman: Two points, first with regard to what Harvey Brenner was saying. I very much agree with him. What we should recognize, what he didn't say, is that there are prevention methods that are appropriate to certain levels of our society. For example, price-prevention programs are appropriate to government. That is the kind of thing that people who get contributions from the State would be very concerned about. At the same time, in terms of what is appropriate to the larger mass of society, heterogeneous as it is, I think that mass-marketing efforts are considered appropriate. If they are not appropriate, why are we wearing what

we wear, eating what we eat, jogging, and so on. These have all been, to me, very effective programs.

Perhaps one of the problems with past efforts in this direction was too much input from scientists on the research side and too little from scientists who understood manipulating people's opinions. We have to close the liaison that took our expert opinions to their expert sources and then proceed with a model of treatment and prevention. Because I think that, in a way, this is a way to treat the already heavy user and abuser, as well as to prevent further alcohol abuse. In essence, what I have done is combine two points by mentioning the appropriateness of price control, and perhaps even of production control, in certain governmental contexts, as methods those in government would regard as structurally appropriate to them—they are not supposed to meddle with people's minds, they are supposed to meddle with laws, prices, etc.

Griffith Edwards: The notion that Harvey put forward, that media can produce vast changes—that we eat, drink, and have our being as a result of the way the admen made us—is a strange idea to me and challenging. But I would point out that changes in fashion took place before media and television were invented. I believe the top hat was surrendered and the bowler put on because of certain trend setters in society rather than because of television. Industrialization, sharing of wealth, the influence of war are more potent than the advertising man.

We must take a discriminating view of what advertising can do. We may be going absolutely on the wrong track by drawing an analogy with "what we eat, and how we dress." We should note that America still has a high crime rate. And so far as I know, in most areas, there's not much evidence of its having fallen. Now if we want to test the power of the media, let's test it out on the prevention of homicide, of pickpocketing, or of fraud. What is staring us in the face is this much more apt analogy. Socially troublesome, destructive, self-hurting behaviors do not appear to be nearly so readily susceptible to media intervention as do some other things. Egg sales may have fallen. So far as I know, the standardized mortality rate for male adults has not shown much sign of decrease over recent years in most industrialized cultures. I would plead, therefore, for some caution in reverence for the media. Dick is absolutely right in saying that a cultural perspective is inescapable; that like gravity, it is there. Of course he's right.

Now what is the significance of that statement to what we can do to stop people hurting themselves, to aid people to stop hurting themselves? I think what social science may first do is to work on

the whole climate of ideas in which we operate, and that is important. It serves to make society a little bit more humane. Auden wrote a poem on Freud in which he points out that Freud made life potentially slightly less vile, that he altered the climate of our civilization, the view of the child, and made forgiveness of ourselves more possible.

Similarly, I believe that what social science has contributed will affect the whole climate of ideas within which we grapple with drinking problems. And that's not to say, "There, there — pat on the back, boys; you're not wasting time." It is vital to understand that we're not ever again going to see the problem of alcoholism as residing in the individual alone. Perspectives and understandings have been much enlarged by the whole series of contributions in social sciences.

Now beyond that, I worry about the "size" question. Because of the humane implications, that particular awareness, the social scientist may believe that he is ever forced to address the large questions. He is a nothing person unless he takes the world on and seeks to right the large wrongs. Well, I'm not sure that's right. I'm not sure that is what necessarily stems from the awarenesses we have been given.

For instance, we will have to admit that it is not given to us, it never was given to us, to reverse the processes of the industrial revolution. If we had a workshop of social scientists in 1840 on how to reverse the processes of the Industrial Revolution, I dare-say we would have had very little impact. Others might take a different view and believe that the Industrial Revolution could have been halted. I doubt it. I think there have been reminders that others see alcohol for different purposes—society is engaged in the pursuit of pleasure and the pursuit of profit. And we simply haven't got the power to carry through the implications of our researchers to altering the larger shape of society. You may feel that that is a terrible surrender. In some small part of my activities on certain occasions, I go on trying to influence the shape of the society I live in. But I don't expect that I will have vast successes during my lifetime. That's not to dismiss the matter; we want a more just society.

If we say, "The way to deal with this problem is to have better education, to get rid of social injustices, to avoid slums, or to avoid disasterous levels of unemployment," I think we will find people in government who are as concerned as we are. If you knock at the doors of the appropriate ministers, you will find that their whole reputation, their ability to get themselves re-elected, depend in many countries on their efforts to deal with

just those problems. If you go and tell them you've got to have better housing, this will not be hot news for them. "We must not have slums. We must not have ghettos. We must not have unemployment." That message will be courteously received but not seen as an immediate basis for policies other than those society is now stumblingly pursuing. Nothing I have heard alters my view that social science had best—while continuing humanely to infect the atmosphere of awareness and to provide commentaries on the shape of society—that it had really best attack segmented problems. I see no vileness in this; I see no betrayal of larger principles. Indeed, I see a danger of the misinterpretation of social science as being grandiose—peddled as the opiate of the intellectual classes.

Program Implications: Prevention. Roles in a Game and a Way Out of the Game

John R. Seeley

I gather I am supposed to make some remarks on the "implications for program" of what I have heard (and perhaps not heard) in the various papers and discussions under the rubric of Normative Approaches to Alcoholism and Alcohol Problems in the last few days. By "what I have not heard," I mean both what I may have misheard and what I might have said in any case, absent papers, discussion, and "informal participation." Because these are difficult to disentangle and there is no obvious fruit in doing so except to engage in a new debate, I shall, unless unduly pressed, leave them entangled, i.e., I shall not emphatically distinguish the conference as cause of—as against occasion for—certain remarks.

I should be delinquent in my duty, I think, if I did not draw first to your attention and my own, three aspects of my task, each of which I find rather striking. First the forced divorce, by separation in time and space and by incarnation in two distinct officiator-respondents, of theory (and its "synthesis") from program (and its "implication"). This already marks a particular, if not a peculiar, view of the world, and the relation of theory to praxis and praxis to theory. If, as I hold by honest inheritance, the object of understanding the world is to change it (for the better), if indeed understanding is, whether we like it or not, not the prelude to change, but the first and decisive act in the change itself (for better or worse), then the understanding to be sought is governed in terms of final cause—ought to be governed—by the end-in-view at least adumbrated and clarified as the desirable and feasible beneficent change. So the synthesis of theory—or its inauguration, establishment, or reestablishment—cannot proceed properly prior to the clarification of ends-in-view and their embodiment *in statu nascendi* in an ongoing historical process of being brought to birth. As I said yesterday of research, so I say today of theory (and its inauguration, synthesis, analysis or what not—

all theorizing), it is the pursuit of politics by other means, meaning here by politics both the ordering (and creation or recreation) of a polity, and the power-based and power-consequent struggle of who is to get what (to paraphrase Lasswell) of what there is to get. Surely it is clear that the theory we need is the theory appropriate to the world we envision (or the world we *ought* to envision), and to the transition or transformation that is feasible in historic reality and that, as *integral to the ends* sought, is appropriate to and best guarantor of an evil-minimizing "transition." Surely it is also clear that the theory *is* (for better or worse) the transition. Someone pointed out yesterday that "culture" as a term of anthropological and social art is—I would hold, with somewhat disastrous consequences—a commonplace now of everyday speech, and a not inconsiderable causally-operative (or co-constituting element) in a thus changed or altered culture in which certain outcomes have thus been rendered more or less probable, or have become respectively possible and impossible. But that very remark—as a theory—will in some virtual tomorrow exist as a powerful consideration, a something that will already have altered the terms and conditions of social action, society, and culture. Culture there is already by that theorizing incipiently a doubly reflexive culture at least—yesterday a culture conscious of "culture" (and using it principally as a defense against political challenge and being weakened and having its culture-bearers depotentiated and weakened as political agents thereby); today a culture conscious that its recognition of culture is a distinguishing feature of it, and certainly changed, but—depending on specifiable circumstances—further or nearer some envisionable better state accordingly. Neither "in the beginning was the word" nor "*Im Anfang war die Tat,*" but "*Das Wort ist die Tat,*" "*die Tat ist das Wort,*" the theory has already wrenched the change before it can be asked what ought we then to do.

I will make one more linguistic observation on the task-definition. The words "implications for program" are highly suggestive. Certainly "program" suggests—not necessarily by etymology, but clearly enough in context—some relatively quiet, controlled, rational, miniscale or medium-scale, establishment-fundable (and directible) articulation of manageable elements directed by disjointed incrementalism toward some minor enhancement of some good or some minor diminution of some evil, in some orderly and domesticated way, not reaching to a radical and totalistic transformation of a whole way of life—neither a cultural, nor economic, nor social, let alone political revolution, say. It is not clear—absent other presumptions unspoken—why this should be a

"reasonable" or "good" or "right" or even feasible approach. Given what the Jessors report as to the "distal" effect of what they class as "Demography-Social Structure" and more "proximally," "The distal structure of the perceived environment system" (curiously foreshortened to exclude perception of socially structured mass social injustice), and given what we know from other econometric studies of the effect of the latter perceptions (mediated by reference-group self-siting, personal and collective) on violence, especially supposed gratuitous violence, turned directly and indirectly on the self and others, of which alcohol-behavior may be a subspecies, one might have thought it all but self-recommending that the way to go would be to publicize further to those evidencing "problem behavior" (in their vocabulary) the so far somewhat feeble connection between the injustice built in (their victimization and exploitation and that of their significant others) and their behavior, thus probably turning (or tending to turn) self-pity, self-solace, self-punishment, self-destruction, personal and collective, toward anger, fury, struggle, first perhaps expressive and alarming to the establishment, then quiet fury organized into the unity of the hammer for effective overthrow of the basic and continuing conditions, conciliating minor differences that aid the principle of *divide et impera*, so that the singular line between (neglecting fine shadings) oppressor and oppressed becomes the battle-line, the principle of unity on both sides—where do "we" belong?—and the means of, if not eliminating or mitigating "the problem," both putting it in its minor perspective in the scale of importance and making evil serve good—the best we can usually do—by letting understanding of the problem, properly located, serve the ends of much more inclusive, indeed overarching goods, such as social justice, or if that is too high-flown for you, a world not founded in principle on organized greed, enmity, and, where that is not true, anomie (aimlessness, except instrumentally) and ultimate (i.e., fundamental) absurdity. But this—we could at need set the ghetto afire or better afoot with such education—is not, I am sure, what my employers, my friends, and colleagues mean by a "program." Though I say to you, in all conscience, that I doubt if I can regard anything seriously different as either serious or morally or ethically permissible, even if feasible. I put it to you as a canon of conduct that nothing should ever be done—though I cannot sometimes help at microlevel doing it—to meliorate petty miseries except in so far as they directly, manifestly, and massively serve the destruction of the general system of (in the United States, velvet or silver) enmiseration, or, at the lowest, neither

disserve its destruction nor cloud nor mystify the system's operation, its centrality, and its ultimate determining of the distribution, existence, and effect of minor miseries, and such major ones as the universal induction, beyond existential necessities, of the fragmented, riven, and essentially nonhuman life—with or without the comforts and cossetings of marginal rewards and formal "freedoms." I do not rule out psychotherapy, small group therapy, or "education," or intermediate tinkering or attempted tinkering with belief-systems or what (horrible jargon) are called behaviors (a degenerate term, reductionist of "conduct"). The problem is in a general and classically recognizable form the relation of "reform" to "revolution." They are neither, as vulgar Marxist dogmatists would have us believe, inherently opposed (reform blunts the edge of revolution) nor providentially convergent. Only a coherent scheme—theory, view, and plan—and an historically persistent vehicle, appropriately allied and materially based—can see that the disjunct benign incremental victories, even if appropriately achieved, both cumulate and become the ground and source of strength for the kind of change that could give meaning again to life and justification to these efforts (otherwise, quite possibly reprehensible).

The last word I want to deal with is "implication." I do not believe that any program or line of action for anyone is strictly implied in the papers or discussion. If anything is implicit—in the sense of taken for granted, rather than an implication as a forced, or all-but-forced conclusion—it is, or appears to me to be, that something like what we are doing, perhaps continued, expanded, and refined, in something of the manner, and somewhat within the same limits we were doing, is allowable, mandatory, or to be desired. I question all three—for a vast variety of reasons of varying reach and scope. When Sherri Cavan, in a genuine and searching *cri de coeur* asked, "But what are we trying to prevent?"—or do—I said I did not believe we were trying to prevent—or do—anything (except perhaps very comfortably survive), and further that whatever we might think we were doing or "preventing," what we were acting in, consciously or not, willingly or not, was a "prevention scheme"—which is a very different scheme from anything remotely likely to prevent what it is formally said to be intended to prevent. We are no more intended to prevent whatever it is—even that is not clearly focused—in the sense of removing or radically reducing or abating it, than the police are to prevent crime, or doctors are to prevent disease. We are to *Maintain* a preventive effort, i.e., carry on a moderated and controlled warfare against those given the means, subsidized, aided by essentially

the same employer, to maintain the warfare, also without victory, so that an essentially diversionary spectacle (diverting both in the sense of entertaining and in the sense of displacing effort from what matters to what doesn't) may be maintained, go through its variants, an analogue of TV cops and robbers, provide vicarious thrills, intellectual and good-against-evil dramatic, provide communiqües and dispatches, hills captured and valleys lost, but never, never, to win or lose the war—unless some really high-order social necessity as seen from the perspective of those who "naturally" rule the system should decree otherwise.

The view that something else is afoot—that we would be allowed to do or conceive or propagandize for something really effective—depends on a radical misunderstanding of what is a social problem, what, in a different vocabulary, gets to be accredited as and thereby becomes a social problem (such as alcoholism or substance misuse or delinquency or crime). In an entrepreneurial, industrial, bureaucratic society, as I have often said elsewhere, and as must be obvious, nothing can be or is accredited as a social problem that cannot, as one criterion, bring into being and maintain on an adequate scale at least six interconnected industries. There must be an industry that creates, maintains, and, within limits, expands the problem—an organized crime industry, a distilling industry, a drug industry, a mystification-industry called school, a sufficiently effective psychotogenetic organization of work and everyday life—whatever you will, the supposed "problem producers." A second and a third industry must be called out and must be elaborated and substensible in the way of a hard and soft, respectively, police or counterforce industry, suitably matched and adjusted to maintain the problem at just the right height of prevalence (and incidence). The hard police industry is normally the recognized police, and hard-nosed professionals such as psychiatrists and teachers acting punitively in their place or as reinforcement. The soft police industry—to divide the victims further, and to institute in effect out into society a graded minimum-to-maximum security system—consists of non-hard-nosed professionals, such as psychiatrists, psychologists, social workers, nurses, counselors, acting in their one-kind-of-punishment-remissive character, and recruiting out of the consumers for industry one (industry one, recall, is the covertly approved alcohol, drug, crime, breakdown-whatever—"pusher") those whose public service is to demonstrate secondary compliance (needing "help" rather than primary minor defiance (demanding their carefully induced fix). The fourth indispensable industry—to help explain away the otherwise incomprehensible complexly-organized

deadlock—is the research industry (ours) to try to uncover what the previous industries and their sponsors have set in motion and covered up. (I must qualify this—we are not to spoil the game, uncover the whole system, but like good chess players in a modified endless chess game make moves that have a strategic air, met by countermoves, presenting at microlevel ever-fresh problems). The fifth major industry that must be inaugurated and sustensible is an education and/or education-plus propaganda industry that carries the contentions of all parties, suitably mixed and balanced, into a cacaphony that has invidious effects of its own, but primarily into a mystification system that, again, permits shifting about in particulars (guerrilla and tactical sorties, strikes and reconnaissance-patrollings) while leaving the “problem” substantially as it is, except now further indurated, by convincing those educated of its complexity and intractability (or, falsely, but to the same effect, its simplicity and, but for the unreachable bad guys, tractability). The sixth necessary party is a major industry of entertainment and communications, to market the several cops-and-robbers games—will the cops catch the (little) “pushers”?—will the psychologists spot, capture, and save the potentially contrite?—will the researchers find and at least identify, if not indict, the noxious agent?—and the high entertainment and gratification of the hunt, the capture, the trial, and the punishment (which spices our daily lives with vicariously enjoyed cruelty, and if punishment is ever justified, turns our eyes away from those we might properly, if not wish to punish, either restrain or be revenged upon). I do not think I need to add as a separate industry the bureaucrats, think-tanks, cabinets, or other coordinators who facilitate this permanent play.

For note that durability—preferably, perdurability, permanence, problem-insolubility—is of the essence; it must be built in at the beginning or provided by elaborate negative feedback. No such industry can be mounted just practically for a war of brief duration. People need tenure and career-expectations in every one of the industries involved. That requires problem-stability at least, and problem-expansion along in-built imperialist lines, if possible. I cannot fill in here the variety of ways in which duration—if possible, permanence—is assured to the problem. The ideal way is by stating it in insoluble terms as in the case of “mental retardation.” An alternative safeguard across problems is to insure that persons-with-problems simply escape from one problem category into another, basically on an exchange system. (Prisoners go into “mental hospitals,” “mental hospital patients” become “community mental health center clients,” their “outpatients” become “al-

coholics" or "addicts," the latter cease and become "child-abusers" or "hypertensives" or "stroke," "suicide," or "accident victims." Note that at every transfer point, as with a transaction or income tax, the employer-of-all-ultimately, the State, counts coups, and counts each *transfer* a success (an alcoholic "cured," a prisoner non-recidivic . . .), a sort of social-psychological gross national product. Note also that as with a fixed supply of money, even if transactions can be *speeded up*, the apparent income per annum (what the government can take credit for) increases. With finer division in categorizing problems we could increase this specious but apparent "income" indefinitely.

I want it to be clear that no matter how good our intent—I do not wish to make my good friends feel bad—our role in the play as a whole cannot even faintly resemble our good intent. If it did we should collapse the enterprise that supports us, defeat the State's interest—said yesterday (dubiously correctly) to be less specific than, say, the wine-industry's—and collapse the society as we know it. As things are we are agents in—and as necessary to as any of the other parties—the overall game, which has as utility on one side the stabilization of the system as a whole, and on the other the recruitment into mutually defeating warfare (in the service of all the parties involved) of the brightest, the best, and the most committed, who not so diverted, and otherwise engaged, would seriously threaten the very foundations of the system, even if only by adequate understanding of it, gained from and feeding into a very different praxis.

I do not—if you are listening any longer—know how to tell you to go from here—what are the implications, following even this cursory analysis, of what we have witnessed, seen, and heard ourselves and each other to be doing. But it is obvious—at least to me—that nothing at all can be done, absent a clear and continuing recognition of the role we play, our complicity thus in the perpetuation of the problem we purport to deal with, and thus our role as stabilizers of the going social order, or at the very least such marginal changes as it is willing to make on the principle of the *minimum feasible ransom*.

I want to make one more point, in any case, before passing on to the question, answerable or unanswerable, of what is to be done. That point is my conviction—not original to be sure—(1) that the summation of innumerable partial critiques is not the same as or in any way similar to a critique of the whole; (2) that only a critique of the whole gives us an appropriate point of departure—a warrantable foundation—for a non-trivial critique of anything, say some part; (3) that absent a critique of the whole,

and a continuing institutional body to sustain the critique and the critics in it, there will be no such global critique and hence no warrantable partial ones; (4) that absent such a critique, we increasingly fragment life, rob it of meaning and hence of the sense that anything is intelligibly better or worse—while claiming that that is the cause rather than the consequence of our hypocritical stance; and (5) that only in the search “single heart and single sword” for such a critique, will the dialectical process proceed in which theory and praxis form and inform each other, and the critique enters as a circumstance both into circumstance itself and criticism per se.

The first implication—in the strong sense now of a necessary and unique conclusion—of these general, if sketchily adumbrated views, *and* the performance put in evidence—as typical rather than abnormal—at this (“very good”) conference, is that there are no implications of consequence failing a new understanding of our objectively effective roles and a determination radically to alter these, *and (Wer A sagt, muss B sagen)* the establishment of the material and social conditions that would make such a determination other than an exercise in “idealistic” (in the pejorative sense) futility. The material conditions entail at a minimum that we come into control ourselves of the means of production (in reference to our product) and of reproduction (in reference to our successors). The notion that we control the means—and relations—of production, intellectual production, in an era of grants and contracts, stemming from one confounded state-military-industrial-academic-idea-manufacturing-and-disseminating complex is surely too fantastic even to invite passing belief. The notion that we have control over our own reproduction, given the mega-institutions of research and higher learning—preceded by the castrative-eviscerative functions of the megainstitutions (“school system”) of the lower learning—is at least equally fanciful. We are a-politicized, de-politicized and out-politicized, *clercs* who know no *trahison* because we were so distantiated from the political as to know no overarching loyalty to *betray*. And as to the establishment of the necessary social conditions, that means finding new allies—and hence new enemies—and again any notion that we can do so in the “value-free” cloisters of academia or the conventional research community or “learned society” is so figmentitious as to be beneath comment.

The first task then is to begin to take ourselves and each other seriously, and that entails sorting ourselves and each other out politically (in the widest sense)—and clinging to our political allies and engaging our political foes. We have been conditioned or

brainwashed to "repressive tolerance," or if you do not like that Marcusian term, to what the Catholic church calls indifferentism. Value-free science and value-free inquiry are ideologies. But the next line of ideological defense, value-laden science and inquiry, informed by minivalues and micropolitical perceptions or values may be even more destructive. The "scientific community" and the "research community" are as much myths as a postulated community of German Nazis, German Gypsies, and German Jews. The scientific community is divided at deepest level on the very nature of science, and on the next level on its appropriate questions and permissible and mandatory uses. And the fact I assert is made difficult of recognition because we *are* allowed our marginal ethics and their attendant marginal freedoms. We do not have Lysenkoism. We are not driven to hew to or forced to abandon, over a wide area, any line of inquiry. But we do live in an aura of petty pragmatism. We cannot find funds or support—or indeed institutions and companions—for the pursuit of the links in the causal chain until we reach those links that conjoin all else to the fundaments of the system—conquest, cumulation, control, cannibalism (the technarchic reduction of others to objects of control by ego, and of the rich self to control by the narrowed executive ego).

When we have sorted ourselves and each other out politically and found our necessary supports and allies, then the nature, means, and degree of interference in the lives of others, with respect to what, in what way, in what order, permissible or mandatory, will begin to clarify as elements in a political program in which we shall be openly and recognizedly (and therefore vividly cognizably to ourselves) in struggle with ourselves and others. In that struggle, praxis will take form, growing out of political aim and strategic appropriateness, and therewith appropriate theory will emerge, integrated with overall theory and not disjoint, like a nostrum for every complaint.

We may then begin to call ourselves scientists, I believe. Or at least serious enquirers. And only then will our praxis come close to the maxim "at least, do no harm." As we are, though subjectively innocent, we are I believe noxious in our efforts, and irredeemably so. It would be strange if among those noxa, we did not substantially affect for the worse those uses of alcohol that add to the level of misery or embody it and symbolize, albeit mutely, a but half-conscious protest.

Feliciter eveniat!

General Discussion

Harvey Brenner: Many, many fundamental questions were raised by the extremely well-tracted dissertation by Dr. Seeley on the nature of science, the meaning of program, the relationship of social justice to the use of psychotropic substances, and so on. And these topics have been raised, with not quite the eloquence and force that Dr. Seeley brings to us, but have been raised from time to time. I think there is something of a problem that we have forgotten several times during the conference—the problem of the relationship of science to politics. There seem to be two camps: one assumes lack of evaluations, the other assumes the necessity of it. If we assume that we are to use our social values in the construction of our methods of research, then we will get nowhere and the research will be useless. We cannot enter the value system into the conduct of the research enterprise itself because different peoples' values lead to different results, and not only will they not come to any program or policy or sense of what must be done to change, but there will be no agreement on what the basis of true value is. On the other hand, it is certainly the case that all problems—all research problems—originate with some valuating point of view. And it is certainly the case that all implications of scientific method would lead, must lead, to a vantage position that one must take.

On a question of program, I must agree very thoroughly with Dr. Seeley: the term is most inappropriate for social problems of the magnitude of psychotropic drugs, including alcohol—for social problems in which one can observe massive changes in society that have to do rather strictly with changes in the economy; these changes are regularly under the influence of our own government. What changes bring about the economic policy that we usually have? The point is, I think, we must go far beyond a question of finely crafted programs to a much more general policy having to do with the well-being of the citizenry in general—having to do with the kind of things the Jessors spoke about, the key issue of opportunity, the quality of opportunity across social strata, and the key issues, once again, of the processes of distribution. These things have to be made very explicit in our own social scientific view, if that's what we think we're holding, of problems that are so massive as those connected with alcohol.

There's just no getting away from it. It would be a disservice, I think, to our government and our people if we were to make programmatic statements that attempt to have them believe that

the problems we are talking about can be handled piecemeal, or chipped at incrementally. I don't believe that, and the historical evidence is fairly clear. Quite obviously, a number of approaches are needed, some perhaps programmatic and some that can be quite deliberate and systematic, of the mass communications sort. This is something we say over and over again, and I think we'll leave it be at this moment—except to say that the implications of the media are very, very profound within the economic and social systems as a whole.

Beyond that, there are a great variety of curative as well as preventive moves. A question before the public-health-oriented group altogether is: What is the proper role of the physician? Let's take the situation of smallpox, a large-scale problem in society when it is rampant. What is the proper role of the physician? How shall we extend our traditional view of problems? Shall we view it one case after the other? Or shall we view it with isolated preventive strategy and thereby catch the cases? Well, the answer is yes, we have to do both. Because there are, in fact, people with smallpox who are likely to die. And when the person presents himself or herself before the agency for treatment, it is the obligation of society to deal with that person. At the same time, it is the obligation of society to deal preventively so that no more people are hurt. I do not see that a series of policy implications that would involve us in national and rather massive cultural change, which is probably quite appropriate over the long term, is mutually exclusive of a smaller, more easily tracted sort of approach. In fact, one can probably construct programmatic approaches that ultimately add up to rather considerable policy changes. This has been true for many years in the field of macroeconomics, where, indeed, there has been considerable alteration in the distribution of at least some elements of the wealth in this country.

Joy Moser: I think it's not necessary to be quite so pessimistic about the program implications of what is being discussed at this meeting. I would rather like to look at what have sometimes been seen as polar aspects of these problems. That is, the sociocultural model and the distribution-of-consumption model have been rather similar to, first, the medical model and, second, the public health model. I think there are possibilities of working on the prevention of alcohol-related problems from the public health angle—that is to say, attempting to reduce the rates of problems, or at least minimize the problems. Then I think if we look at the history of what has gone on in public health not only in this country but in the world in general, there have been tremendous changes in the last 20 years or so.

Since smallpox was mentioned, I might just remind you that not many years ago millions of people were still dying of smallpox. Now, thanks to a big international campaign, there are very, very few pockets of resistance left in the world, and that only in one country. Soon smallpox will disappear entirely as a disease, probably. There may be a rebound, of course.

The fact that the distribution of consumption model has been resuscitated is hopeful. It is very interesting that it has been discussed mainly in Canada, where the people concerned with research are at the same time concerned with the application of their research findings. Maybe over here it will take a little longer to apply, but I think research workers should be quite capable of looking more closely at what the implications are of attempting to change the availability of alcohol—the implications not only on the national level, but at State and local levels. Which powers will have to be fought? Which are going to be the best socioeconomic methods of getting at the problem?

John Seeley: I do not *wish* to see those rates reduced, if it is avoidable, except in such a way that the reduction of pain, or amelioration in the number of ills, cumulate consciously and on a sustained basis into some things that alter the very foundations of the system that systematically produces such ills. Only so, I take it, can anything consequential be done.

Those are all political questions. I do not want to separate—because that *is* the liberal fragmentation—I do not want to separate, if it is at all possible, the various components of the problem from the problem of altering the system as a whole, because I do not want people who need a loaf of bread to be put off with a “successful” diet of food that fails to nourish them.

Wolfgang Schmidt: Let me just ask you one question. Let us assume that a dear friend of mine is run over by a car that's driven by a drunk driver. I am in despair, and the reply I get, “We could have prevented it,” but in so doing we might have strengthened the system. It creates a dilemma for me that is very difficult to live with.

John Seeley: You have told me elsewhere beautifully and dramatically of a very real problem. And I did say in the paper that no matter what else I've said, and no matter what contradictions it involves, I would give help on the terms opportunity offers. And so I would, if I could do it, I presume, try to save your friend's life, even if in some minor degree that strengthened part of the system. But I do not regard that as a justifiable basis for policy or policy evaluation; I regard that as a concession to my weakness by way of short-term sympathy, warring as it must do (and that's

a paradox, warring as it must do), against my desire to see an improvement that will really matter in the long term. I cannot sacrifice either tail of the distribution of possible acts completely.

But what I am asserting is that the whole effect of the set of enterprise that nearly all of us are engaged in—the most obvious effect is: "Keep the major problem out of sight and I'll keep them preoccupied with relative trivialities; deal with the problem, if possible, one case at a time." I can't refuse the reality of the tragedy, and I would do what you would do: I would try to save my friend by any reasonable and available means. But again, I said that in stating the canon. I said, "Where evitable I would not wish—*where evitable*—to let meliorations come under the wrong auspices, or detached from each other, or in such a way that they do not cumulate, and are not seen, really to be minor skirmishes in a war, whose aim is declared and shared (or not shared) by people who are defined as being on our side, or on the other side."

Reginald Smart: Jack, I found your presentation very amusing and entertaining at point, but I also found a rather depressing view of the world we all live in. It's not a world I recognize for the most part. Your presentation, I think, didn't help me very much, and perhaps in your reply you can tell me that I'm wrong, or that I should recognize the world I live in.

I wonder, where did the assertions come from? Are they theory, or are they social criticism? Are they based on some empirical approach? That is, that social problems are there to be kept going by a set of industries that are sort of on the periphery, with ourselves being part of the industry. I really don't recognize that in the area of the world I come from or the people I know who do research. I think that myself, I'm an extremely altruistic person, and so are most of the people I know. I think that we are around to really try to solve the problems. That may not be why we are being paid by people in government. But I do think that crises ought to be seen on different levels. They may pay us for something we don't do. And we may do something we're not paid for. So I think there are several kinds of levels to consider on.

Also, I'm not convinced that even the liquor industry would not want to see alcohol problems disappear. When I talked to them, they would love to see alcohol problems disappear. However, they're not willing to go to any lengths to have that happen. Alcohol problems, traffic accidents, and alcoholics are a tremendous embarrassment to the industry. They would be glad to see all those things go away, provided it didn't affect their profits very much.

I'm not sure about a conspiracy with these six kinds of indus-

tries or groupings. I think that we have to treat effective cases, and you've already said that that's the case. I don't really see any evidence or any historical evidence at all that any sort of "radical taking to the streets" approach would really be better at this particular point in time, or any revolutionary approach. I think that one difficulty with problems in this field is that we've not had much to tell the government in the prevention area until the last few years; that is, anything to tell the government that people could agree on in the scientific field. Up until a few years back, 5 or 10 years back, about the only people around who were interested in this distribution approach were people in Ontario and perhaps a few Finns who didn't speak English, for the most part. Now, it would have been very difficult, I think, to get government to do anything really spectacular, given the large amount of disagreement in the field. That may be seen as some sort of conspiracy on all our parts, but I am really not sure of that.

John Seeley: Surely you will not claim for yourself any unique insight into what we were doing for the most part before you came to the Foundation. But when you speak of a "conspiracy theory," you've injected into the paper something that was not even faintly there or remotely suggested. It was merely suggested that nothing will get to be a recognized social problem unless it has a certain durability. And this is based on observation, and I suppose cast also as social criticism. You question whether or not this is a theory that has been "tried." I would think the evidence for it is available by simply looking at both agency after agency after agency and program after program after program, and observing the points at which people back away from anything that might be a potential solution to such problems. I would only say that in an industrial-entrepreneurial, division-of-labor society, such an arrangement serves the stabilization of the system. It is soon seen that by in effect increasing the expenditures simultaneously on all these matters, you satisfy more and more constituencies, and nobody complains because you can point for each separate constituency to a different aspect of it about which you are "doing something." Something like Johnson using the Peace Corps on one hand to offset his hands dipped in blood on the other, also "pacifying" some other country. And it's that kind of observation—whether you agree with me or not—that kind of observation made time after time after time that I would have to regard as furnishing the body of the evidence. Certainly, it is also a social critique.

Reginald Smart: I think there are more bright spots than I heard in your presentation perhaps. There are areas that are now

taking a good look at alcohol policies. I think there is a moratorium in Ontario. And I think it probably took too long to get there. I think there are certainly some provinces and States that have changed the age law back again. Perhaps these are only small changes that would not be the kind of major change you would like to see, but I'm not sure what evidence we have that a radical revolutionary approach would be better. I don't know we know that.

Richard Jessor: I do think that we have some examples in this field that are germane to the issues Dr. Seeley raised. If we take nothing else from what he's said, we should be aroused in our own self-consciousness about the kinds of decisions we make.

Let me just take this example: If one looks again at Wilkinson's report about the choice the Cooperative Commission made in advocating a policy position, he makes it very clear that the decision the Cooperative Commission made was not based upon the research evidence but was based upon the issue of its perceived feasibility. The notion of perceived feasibility is a notion that is almost automatically captured by the nature of this status quo, the political structure at a particular time. That is, it is a judgment about what can be done that would be acceptable at a particular point in time. That's quite a different position than to say what might be done if the situation is changed.

I think the issue Reg raised this morning, that governments need to be told things and "you tell them what you can tell them," itself is problematic. It requires researchers to ask, "Have we asked the right questions ourselves so that we can tell them other things as well?" Politicians *have* to act on the basis of feasibility. They *have* to be pragmatic. But that doesn't mean that, as social scientists, *that* should define our recommendations. Our recommendations ought to help politicians to enlarge their perspective, to guide them to try to alter the conditions of feasibility rather than follow their definition of what might be practical and feasible. You're absolutely right in saying that they may not implement what we tell them if it's not going to be manageable. That doesn't mean that we ought not reshape the context of decision making by suggesting that other things might be done if that context were changed. So I want to support that aspect of Dr. Seeley's comment because I think we all tend to operate out of a level that's less self-conscious than it should be.

As researchers, as people concerned with prevention, I would like to urge that we enlarge our perspective beyond the definition of what might be practicable or feasible and try to tease out all the implications of what our understanding of what human be-

havior is, and the ways in which social conditions and social change can alter human behavior. Now that may sound like it's radical. It doesn't seem to me to be radical at all. It seems to me to be inherent in the very definition of what research is all about. I think that's our task. We ought not to be camp followers of bureaucratic decision making. We ought to help the bureaucrat by saying to him, "Here's another perspective. Try to use it if you can. Try to implement it if you can. But we don't want to give you the same perspective that your own colleagues can give you."

John Seeley: I am so fully sympathetic that "No comment is needed." There was no suggestion, let me clarify, that we would do nothing "while we wait for the revolution." What I asked for was that good be done, but *if avoidable*, it not be done outside some program, some enlarged vision of the kind of better society, better world, and better life that we wanted and to which these minor gains would be related. And the program and vision should be carried by a *continuing* body of people with other aspects of the battle in mind, who would make those things accumulate into some structural alteration for the general good. There was no suggestion of not doing anything. As for what I meant by "revolution," at one point I meant the difference between something that approaches things in terms of a critique of the whole, instead of merely a critique of elements, because that is simply mystifying.

I really like to have it both ways. But I'd like to call your attention to another instance: absent the burnings in Watts, a number of substantial meliorations in the lives of blacks, including the creation of the hospital and medical school in which I work, would simply not have occurred. But again, the principle of the minimum feasible ransom applies: it will be the smallest possible medical school and the most ill-served hospital, but nevertheless both do exist.

Robin Room: I find something to recognize in Dr. Seeley's eloquent presentation. From one perspective, he was reminding us of the intractability of the equilibrium in which we find ourselves, and of which we are a part, between the various forces who have some play in the area of alcohol. Though he might not like the term, he might describe it as a functional interpretation of our activities. And I think it's important for us to be self-aware as researchers, and as government agencies' staff for that matter, to be reflexive about our own actions and our place in the political order—and in fact, to recognize the essential politicalness of any activities that have been reasonably called prevention.

But it is very easy, I think, for a radical analysis to become a

form of paralysis, to become a justification for inaction. And I think we do what we can in a situation. We can't always recognize fully what the effect of something may be; even if we do recognize the larger effect of something, we may opt for the meliorist position. For instance, votes for women may well have put off the revolution, seeing as women were, after all, a conservative force and tended to tilt the political structure in a more conservative direction. But a radical might nevertheless have supported women's suffrage. We have to pay attention to the likely aggregate of the effect of incremental steps. What is the likely effect on the aggregate level? Dr. Seeley spoke of disjointed incrementalism. We can't even count on that. I am conscious of the possible alternateness of solutions to alcohol problems; that is, an incremental step that may help solve one alcohol problem, may increase another alcohol problem. So that incrementalism isn't even much of a surety in our situation.

But on the other hand, I remind Dr. Seeley of a phrase he used in the paper in 1960 about the disease concept of alcoholism as reflected in the WHO definition, when he talked about an "oddment of aggregation." And that phrase I've relished and used on numerous occasions, and found very telling as a description of what seems to me an ill-assorted collection of things that have been added together under the general rubric of alcoholism. A lot of what has traditionally been treated under the realm of one government agency, and under the generalized term of alcoholism or problem drinking, isn't even an oddment of aggregation.

I think it's important to keep in mind not only the political nature of our action but also the symbolic level of action in this arena. When we're discussing alcohol policies, there's a tendency to come down very quickly to the level of what are the pragmatic issues, what are the effects on alcohol problems of this, that, or the other change. For instance, in spite of the irrefutable evidence that the level of drunk-driving casualties among older teenagers rose with the drop in the drinking age—I still don't think I'm in favor, as a personal political position, of raising the drinking age, because alcohol is a symbol of freedom and full citizenship in our society. And I think that the Finns, for instance, who have been quite realistic and recognize the effects of the increasing of availability, on political grounds would not favor now moving to a system of re-discriminating, and again deny the rural areas alcohol stores that are available in the cities—which was the system as it existed before. So that it is important to keep in mind that there are other values involved besides the pragmatic issues of the casualties and the problems that go along with our policies, and that

each of us in our own political system has a different way of balancing out these competing values.

One last thing is that those of us around the table who are in the alcohol field are in fact mercenaries in the sense that we earn our living at this, and that many of us came into it as something we thought of as a job, rather than as something that we came into with a profound belief. The difference between our generation in the alcohol field and the preceding generation, which is seeing itself increasingly as fighting a rearguard action, is that they really came in as volunteers, and as people who believed very deeply, as people who had a considerable effect on the society because of their beliefs. Those who were not initially government-funded were not responsible to the polity as a whole by virtue of that government funding. Although many of them now are government funded and, in fact, there's been considerable difficulty over the question of "What the devil are you doing funding a social movement?" As mercenaries, I think we have to recognize the distinction between our own politics and the politics of the situation in which we find ourselves. From my perspective, there is some necessity for balancing off my own political feelings with the sense of responsibility to the polity as a whole, certainly for those serving in a government staff position, and to some extent for people in research positions. There is a curious process of implied negotiation that goes on (as I think Dr. Smart was suggesting) between governments and researchers over what are the topics of study and to what extent is the researcher going to define the topic of study—given that they are not free individuals with private resources—and to what extent the research topic is going to be determined by government agencies.

I think it is important also for us to recognize the essentially political nature of the situation in which we find ourselves. Certainly Dr. Seeley would recognize (but I think that many in the field do not recognize at times) the difference between the world as we would wish it to be, and the world as we observe it out there. One of the very common things I see in the literature on alcohol is wishful thinking as an analysis of the problems.

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I. Epidemiological Studies Based on Supply of, and Demand for, Alcoholic Beverages

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